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Preface to Volume VI

Upon the initiative of a number of scholars, a History of Science Society has been recently founded in America. Its purpose is to promote the study of the History of Science, and more particularly to support the publication of *Isis*, which has become its official organ. This is very gratifying, especially because as long as *Isis* remained a private undertaking, its existence was naturally precarious. This existence is now assured. *Isis* may slumber yet, but die — never! The patronage of the Society will stimulate its editor and other contributors to do their best to improve *Isis*, to increase its value as a tool, to enhance its scientific prestige.

The Society will be predominantly, but not exclusively, American. Indeed, it is hoped that every student of the history of science will eventually joint it. One volume of *Isis* will henceforth appear each year and be sent gratis to the members of the Society. Members will also be able to buy previous volumes at a lower price (*i. e.*, five dollars per volume, instead of six). It should be noted, however, that the supply of vol. I and II is running very low, especially of vol. I which will soon be unobtainable.

* * *

Bona fide inhabitants and institutions of those European countries whose currency is temporarily depreciated (that is, all of them except the United Kingdom, the Netherlands, Sweden, and Switzerland) will be able to obtain *Isis* at half price (*i.* e., sixty Belgian francs) by application to the Secretary, M. L. Guinet, 30, Av. H. de Brouckère, Auderghem, Bruxelles, Belgium.

* * *

It is interesting to place on record how the History of Science Society was founded. On December 4, 1923, Professor David Eugene Smith sent a personal appeal to a group of some 45 representative scientists and scholars of the United States and Canada, explaining the need of such a Society, and asking them, if they approved of his suggestion, to become members of an organizing committee. Most of them gave at once their assent in such unmis-

takable terms, that whatever doubts existed as to the advisability of creating a new society were swept aside. It was now clear that the creation of a History of Science Society was not a personal fancy but a latent need of our time requiring only some outside stimulation to blossom out. The favorable reception given to this generous initiative is probably due in large part to the fact that an increasing number of men of science and scholars do suffer from the intellectual disaggregation caused by the very progress of specialized research and feel more and more strongly the need of reunion and of fellowship in a common task.

The History of Science Society was actually founded on Saturday, January 12, 1924, when the members of the organizing committee living in or near Boston, together with DAVID EUGENE SMITH and GEORGE SARTON, met at 2 P. M. at the house of the American Academy of Sciences and discussed the formal steps to be taken to complete the organization. Among other things it was decided at that meeting that the History of Science Society would coöperate as much as possible with Section L of the American Association for the Advancement of Science, but would not identify itself too closely with it. It is clear that the Society cannot succeed in bringing together various groups of men if it takes sides with any one of them. It will thus be expedient for the Society to hold some but not all of its annual meetings at the same time as the American Association for the Advancement of Science; it will be desirable to synchronize some annual meetings with other groups, for example, the American Historical Association or the American Medical Association.

As soon as the Council of the Society is duly contituted it will appoint a Committee on Publication which will share with the present Editor the responsibility and labor of editing *Isis*.

* * *

The first appeal sent on December 4, 1923, was confidential. The second appeal, mailed to a few hundred addresses towards the end of January, 1924, was in the form of a printed circular, the text of which follows :

The undersigned members of a group selected from a large number of those who are interested in the history of science feel that it is desirable to organize a society for the encouragement of the study of the subject and for the assurance of the permanency and the adequate financial support of the international publication *Isis*, now so ably edited by Dr. GEORGE SARTON.

This periodical is devoted to the history of science in its broadest aspect and has, for the past few years, been published by the editor under circumstances that would have deterred most other men. The first five volumes, in which some ninety scholars collaborated, contain 3,116 pages, 70 papers, 527 reviews, and 9,196 bibliographic notes relative to the various aspects of

the history and philosophy of the several leading sciences. The subscription price is six dollars a year, but it is proposed to place the annual dues of the Society at five dollars, to make *Isis* the official journal, and to send it gratis to members. By participating in the work of the Society, therefore, we assist in advancing the general cause in which we are all interested, we encourage the work of Section L of the American Association for the Advancement of Science, and we aid in maintaining and in assuring the future of a journal that is recognized at home and abroad as a powerful factor in stimulating the study of the history of science.

Although the world has so many societies of various kinds that a new one may seem to most of us superfluous, nevertheless the support of *Isis* (which cannot at present be accomplished in any other way that seems open), joined to a movement in which so many scholars are interested and to which such a distinguished scholar as Dr. SARTON is devoting his life, affords an opportunity which it is believed will be welcomed in the Americas and elsewhere. Indeed, the hope which has been expressed of an endowment for *Isis* can probably be realized only in case there is an organization of this kind to assure an interest in the undertaking.

It may also be observed that the proposed Society will be the first, on any large scale, to afford a common meeting ground for scientists, historians, and philosophers. Indeed, the study of the history of science seems to provide the only feasible method for bridging the widening gap between the men of science on the one hand and the men of letters on the other.

In case the number of foundation members warrants the organizing committee in proceeding, it is proposed that it elect a president, two vice-presidents, a secretary, a treasurer, and a council, the latter to consist of colleagues in the Society who are interested in the history of science in general and of the several sciences in particular and to represent as wide a geographic distribution as possible. These officers and council members will then be asked to perfect the organization by preparing a constitution, to formally elect foundation members, to arrange for the immediate continuance of *Isis*, and to serve during the calendar year of 1924.

We hope that you will allow your name to be placed in the list of foundation members, the payment of dues to be made upon notice of the organization of the Society. A blank for your convenience in replying is inclosed herewith.

Organizing Committee* :

RAYMOND C. ARCHIBALD, hist. of mathematics, Providence, R. I.
FREDERICK BARRY, hist. of science, New York, N. Y.
FREDERICK E. BRASCH, sec., section L, A. A. A. S., Washington, D. C.
JAMES H. BREASTED, Egyptologist, Chicago, Ill.

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* The circular contained only the names of the members, these names being sufficiently known in America. However, as European mathematicians or astronomers may not know American historians, or European orientalists may be unacquainted with American men of science, even with the leading ones, I have thought it worth while to indicate the field of study and the residence of each. This will illustrate the scientific and geographic comprehensiveness of the Committee.

ERNEST W. BROWN, astromathematician, New Haven, Conn.
GEORGE LINCOLN BURR, historian, Ithaca, N. Y.
FLORIAN CAJORI, hist. of mathematics, Berkeley, Calif.
HENRY CREW, histor. of physics, Evanston, Ill.
HARVEY CUSHING, surgeon, hist. of medicine, Boston, Mass.
JOHN DEWEY, philosopher, New York, N. Y.
SIMON FLEXNER, bacteriologist, New York, N. Y.
FIELDING H. GARRISON, hist. of medicine, Manila, P. I.
GEORGE ELLERY HALE, astronomer, Pasadena, Calif.
CHARLES H. HASKINS, historian, Cambridge, Mass.
L. J. HENDERSON, physiologist, hist. of science, Cambridge, Mass.
ELLWOOD HENDRICK, chemist, New York, N. Y.
ARTHUR JOHN HOPKINS, hist. of alchemy, Amherst, Mass.
DAVID STARR JORDAN, ichthyologist, Stanford, Calif.
LOUIS C. KARPINSKI, hist. of mathematics, Ann Arbor, Mich.
ARNOLD C. KLEBS, hist. of medicine, Nyon, Switzerland.
BERTHOLD LAUFER, Sinologue, Chicago, Ill.
WALTER LIBBY, hist. of science, Pittsburgh, Pa.
WILLIAM A. LOCY, hist. of biology, Evanston, Ill.
DUNCAN B. MACDONALD, Arabist, Hartford, Conn.
J PLAYFAIR MCMURRICH, anatomist, Toronto, Ontario.
JOHN C. MERRIAM, palaeontologist, Washington, D. C.
R. A. MILLIKAN, physicist, Pasadena, Calif.
GEORGE F. MOORE, hist. of religion, Cambridge, Mass.
FRANCIS R. PACKARD, hist. of medicine, New York, N. Y.
VICTOR ROBINSON, hist. of medicine, New York, N. Y.
DAVID EUGENE SMITH, hist. of mathematics, New York, N. Y.
EDGARD F. SMITH, hist. of chemistry, Philadelphia, Pa.
PRESERVED SMITH, historian, Cambridge, Mass.
EDWARD C. STREETER, hist. of medicine, Boston, Mass.
LYNN THORNDIKE, mediaevalist, Cleveland, O.
H. W. TYLER, hist. of science, Cambridge, Mass.
J. H. WOODS, Sanskritist, Cambridge, Mass.

Correspondance may, for the present, be addressed to

Professor DAVID EUGENE SMITH,
525, West 120th St.,
New York City.

* * *

The present number of *Isis* was edited in October 1923, that is, long before the creation of the Society. It was meant to serve as a propaganda number. This explains the fact that it contains only one main article, the appeal written by the Editor at Intervale, N. H., last autumn, and no other original contributions.

Two other numbers are now printing: No. 17 (VI, 2) will contain the Fifteenth Critical Bibliography of the History and Philosophy of Science and of the History of Civilization (to December 1923); No. 18 (VI, 3) will contain original papers by E. W. GUDGER (New

York), Florian Cajori (Berkeley, Calif.), Friedrich Dannemann (Munich), Giuseppe Gabrieli (Rome), E. J. Holmyard (Bristol), David Eugene Smith (New York), C. Schoy (Essen), Oswald Menghin (Vienna), F. S. Marvin (Berkhamstead), G. Sarton (Cambridge, Mass.), Lynn Thorndike (Cleveland, O.). It will contain also the first list of members.

The lack of means had made it impossible to publish indexes to vol. IV and V. It is proposed to complete vol. VI with a cumulative index to vol. IV, V, and VI, which will greatly increase the usefulness of the collection.

Cambridge, Mass., February 1, 1924.

The New Humanism

« Le passé si beau qu'il soit est moins beau que l'avenir. Il ne peut nous attarder longtemps. C'est l'avenir qui importe. Le Nouvel Humanisme est tendu vers lui »

I published another paper bearing the same title (but in French) in *Scientia*, 1918. The last sentences of that paper form the epigraph of the present one. As both papers are devoted to the explanation of the same fundamental ideas, there must needs be repetitions in the second one; I have not tried very hard to avoid them. Some changes and additions were suggested by my experience of the last five years. I ventilated them at a public lecture of the Boston meeting of the American Association for the Advancement of Science, in December 1922.

The present explanation of my aims is divided as follows : I. Principles; II. The unity of knowledge; III. The unity of mankind; IV. The history of science; V. The New Humanism. To this are added two appendixes; 1) The publication of *Isis;* 2) List of the first friends of *Isis*. The first appendix is, in fact, an urgent appeal to American scientists and scholars. Please do not fail to read it.

I. — *Principles.*

The New Humanism, that is, the ideal to the defense of which *Isis* is dedicated, may be derived ultimately from the following three or four principles :

1. — *Human progress is essentially a function of the advance of positive knowledge.* — Positive knowledge should be understood in the broadest manner, so as to include what little is objectively known in the fields of religion, politics and sociology. For example, the greater religious toleration obtaining in the modern world is in some measure a result of our deeper knowledge of the religions

of other peoples; the more humane treatment of various delinquents is the noblest fruit of psychopathological investigations. It is hardly necessary to mention the gigantic improvements due to the advance of the medical and physical sciences. If the circumstances of our lives are now essentially different from those prevailing in prehistoric days, or from those which can still be found among savages, it is mainly to the progress of positive knowledge and of its applications, that we owe it. Various means of communication, each more ingenious and more marvelous than the other, have so-to-say shrunk our planet to such an extent that man can impart his thought and impress his will almost immediately from one part of it to every other. On the other hand, by their gradual revelation of the infinitely small objects of chemistry and physics and of the infinitely large objects of astronomy, men of science have magnified the universe almost beyond the reach of imagination. The wildest fancies of poets seem petty as compared with the astounding facts continuously unveiled by patient investigators.

Whatever material and intellectual progress there is can be traced back in each case to the discovery of some new secret of nature or to a deeper understanding of an old one. As to spiritual progress, that is a personal matter which is largely independent from circumstances. Many dreadful obstacles which jeopardized the felicity of millions of men are gradually removed by a better knowledge of their nature and a persistent application of scientific methods to their destruction. The chances of happiness of these people are thus materially increased. Yet it remains true that the happiness, the blessedness of each person depends primarily on his own temperament and his own innate wisdom.

2. — *The progress of each branch of science is a function of the progress of the other branches.* — This second principle expresses the essential *unity of knowledge*. It is only for the sake of expediency that we do not consider science as an indivisible whole. We are obliged to divide it into various branches each of which becomes the object of separate study; as our knowledge grows, these branches are subdivided into smaller ones, which become more and more independent, and so on indefinitely. In the same manner to understand the organization of the animal kingdom we subdivide it into branches, classes, orders, families, genera, species and varieties. However natural such classifications, they contain a great deal of

artificiality. We might compare the development of science to that of a tree. The branches can not live if they be cut off, and without them, the tree itself would soon cease to exist. This comparison is not sufficient, however, for the branches of the tree of knowledge are connected not simply by means of the trunk or of the master branches from which they originated but in many other ways. Almost every year sees the birth of a new kind of research which connects various branches of knowledge in a novel way (1). Thus the tree becomes more and more complex, yet more solidly unified; we may conceive a state of development at which each bough will be directly connected with every other. The prosperity of each branch reacts directly or indirectly upon all the others.

3. — *The progress of science is not due to the isolated efforts of a single people but to the combined efforts of all peoples* (2). — This third principle expresses the essential *unity of mankind*. I do not know of a single scientific theory which is the exclusive property of one nation, but, even if such theory could be found, it would be easy to show that it involves many other theories which were contributed by other peoples. Thus the tree of knowledge cannot be conceived as being rooted in the soil of any particular country; its roots are scattered all over the civilized world and derive some substance from almost every part of it.

It is very important to realize that the second and third principles are correlative. *Unity of knowledge and unity of mankind are but two aspects of one great truth*. Their correlativeness has been verified a great many times. Each body of scientific doctrine might be compared to a chain or to a combination of chains holding together, but of which the links are very different and cannot be interchanged. As they must follow one another in a definite order, we may imagine that they are numbered. Now the student of the history of science soon realizes that these chains have not been built, as one might perhaps expect it, link after link, in the very order in which they finally appear. Time after time it has happened that particular links or series of links were made, independently, in many places

(1) Examples : seismology; biometry; astronomical spectroscopy; oceanography; theory of relativity.

(2) Or at least of a great many of them.

very remote from each other. Sometimes links would be missing and many years would pass before it became possible to construct the whole chain, that is, to complete the theory. A good example of this may be found in the present development of physics : the theory of quanta offers adequate explanations of such facts as resonance potentials or the constitution of the hydrogen spectrum but is still insufficient to account for many other optical facts. Pursuing our illustration we may say that some of the links of that theory are still missing. They will eventually be discovered, though nobody could tell when or where. It may be in Japan, or in France or in Australia. The pregnant fact to keep in mind is that each theory, or group of theories, each branch of science and, indeed the whole of science, are made up of a series of numbered links each of which may have been found independently from some of the others (possibly not in their order of succession) and in very distant places. Yet finally all these links constitue a chain so strong that it is exceedingly difficult to break it, though subsequent discoveries may oblige one to improve its arrangement. How could such combinations of circumstances ever happen (and they happen again and again apropos of the building up of each new theory) if science was not one single body of perfect congruency and if the men of many climes who collaborate in this grand undertaking were not actuated by the same fundamental motive?

Another set of proofs is furnished by the many cases of discoveries made independently at about the same time by various people working in different places. If such simultaneity had occurred but once or twice in the course of human evolution, we might ascribe it to chance. But it has occurred again and again; in fact, there are but few discoveries which are not the subject of endless discussion as to the respective merits of several authors. Simultaneities as frequent cannot be due to chance; they cannot but be the results of a social necessity. This necessity itself is unconceivable unless science is a structure of sufficient consistency and unless the efforts of the different provinces of mankind are naturally converging.

These considerations lend a deeper meaning to the classical comparison of mankind with one single man, immortal, whose experience and knowledge become indefinitely richer and whose unified thought we call *science*. One science, fruit of an international and ageless collaboration, one single organized body, the common treasure of

all peoples, of all races; in fact, the only patrimony upon which they all have equal rights.

I have assumed that man has a purpose; if he has no purpose, his existence is a mere farce. The reality of that purpose cannot be proved in full rigor, but it may be reasonably inferred from two sets of facts. In the first place, the general drift of human evolution, the continuous growth of science and its progressive unification; in the second place, the psycho-physiological constitution of man, his possession of a thinking machine immeasurably superior to that of other animals, his constant use of it, his intellectual aspirations, his frantic attempts to explain the mysteries of the universe and of his own existence. Obeying to the same irrepressible impulse, I may try, in my turn, to solve a problem which will remain for ever above our ken, and suggest that the specific function of man (that is, his immediate purpose, for how could these two things be different?) is to produce that kind of energy which no other creature can create but himself, intellectual energy. His purpose is to create immaterial values, such as beauty, justice, truth. Above all, truth, for it can easily be shown that beauty and justice derive from it. True relations are necessarily harmonious and beautiful (the beauty of nature is unsurpassed); they cannot be unfair. On the other hand, however seductive lies or other deceits may be, we cannot conceive anything beautiful, truly and permanently beautiful, which is made of them; we cannot imagine justice built upon untruth, or purity upon impurity.

One recoils from admitting that humanity has no purpose but to exist and to perpetuate its existence. Nor can its purpose be to fight, kill and devour one another, nor even to create material wealth, nor to provide more comfort and luxury. All these activities are means not ends; the real purpose is the creation of new values, intellectual values; the gradual unveiling and unfolding of the harmony of nature, the development and organization of what we call art and science. All other activities are subordinated to this great purpose of the race. It should not be forgotten that however trivial this conclusion may seem to the readers of *Isis*, it is nevertheless absolutely opposed to the accepted views. According to the vast majority of men, including a plenty of so-called educated people, the creation of beauty and truth, art and science, are luxuries; artists and scientists are parasites and the more so in proportion of their very disinterestedness and of the difficuly of marketing the fruits

of their work; they may be encouraged, however, even as we cultivate unproductive flowers, they may be petted and honored even as champion dogs or race horses. According to me, on the contrary, artists and scientists are the true creators, the guardians of human ideals; it is they two accomplish the destinities of the race, who justify its existence; the other men are simply making this accomplishment possible; the masters of industry and business, the great financiers are not in any sense the leaders of humanity but its chief stewards.

How can such a deep misunderstanding obtain and persist? How can people disagree so completely on the fundamental value of their activities? This is simply due to the fact that they do not consider these activities with reference to the same extent of time. Most people care for nothing but the present and the most immediate parts of the past and the future: yesterday, to-day, to-morrow. Their historical horizon is limited to three or four generations. They would measure every achievement with respect to the length of their own life. Their attempts to refer events to such a ridiculously small standard can but lead to absurd conclusions. From that point of view, it is clear that the material needs are by far the most important and that material prosperity cannot but be the main goal. When life is so short, art and science are undoubtedly extravagances. *Primum vivere.* But as soon as our horizon extends further back into the past or further ahead into the future, the utter fugitiveness and secondariness of all our material needs, of all our everyday problems, appear in full light. Essential as these needs be for the moment, they are too futile to be self-sufficient; they cannot be aims, but means to an end. For example, my next meal is very important; if some accident would delay it too long, it would soon engross my mind; yet no sooner have I partaken of it, it is not even worth a remembrance. My life's work is much less urgent; if I stopped a while doing it, I would not suffer any harm but if I stopped altogether, my life would cease to have a meaning; I might as well be dead. Or to illustrate my point in another way, suppose you asked me which organ of our body is the most important. I could not answer without knowing your time standard. Do you think mainly of now, then what could be more important than our vegetative organs? For one must live first, and if one's life is suspended nothing else matters. The slightest injury to one's stomach or to one's kidneys jeopardizes

one's happiness and soon reduces one to inefficiency; while people with no brains may be very happy and their idiotic performances may receive considerable applause. But as soon as we think of the future the problem takes an entirely different aspect. For if we used our brains to a good purpose we may live for ever, while nobody will ever care to know whether our stomach was right or not, or how much we enjoyed our meals. The same paradox would apply as well to groups of people, or to nations, as to individuals. From the standpoint of to-day their material needs are foremost and their greatest men are those who satisfy these needs in the most efficient manner. But if we contemplate them from a greater distance, the perspective changes altogether. Who cares to know the great business men and the financiers of Greece or Rome or of the Renaissance? Their very names are forgotten. The very few of them who escaped oblivion, did so only because they patronized the disinterested activity of scholars, artists and scientists, — men whom they probably treated as pets or servants. Yet in spite of the high regard which mankind has for those who minister successfully to its material needs, as soon as they are dead and its judgment is no longer influenced by these needs, it throws them back into the background and calls their servants — artists and scientists — into the center of the stage. The sober judgment of mankind thus confirms our assumption : the few men who enrich its spiritual life are its true representatives in the light of eternity. Are we not right then in believing that it is they, and no others, who fulfill its destiny?

This enables us finally to solve another paradox : how can one reconcile the unity of mankind, which I postulated, with a chronic state of distrust, of discord and war, alas! but too obvious? Quite simply : the unity is hidden but deep-seated; the disunity, widespread but superficial. The unity is felt and expressed primarily by the few men of all nations whose aims are not selfish, or provincial, nationalistic, racial or sectarian in any other way, but largely human, the very few men upon whom has devolved the fulfillment of mankind's purpose; the disunity, the antagonism of interests, is felt and expressed by an overwhelming majority of other men. These realize intensely that their interests are different from those of the men who live in the other villages, or across the border, or who have joined another party, or who worship at another church. Jealous love of their own brethern; contempt, distrust or even hatred

of all other men is one of the emotional sources of their life, one of the stimulants of their activity. These strange feelings are reënforced by what little historical knowledge they may have. Indeed historical learning and teaching has dealt thus far largely with the most obvious and noisy part of human evolution, but the least important. In spite of many appearances to the contrary, man's essential purpose is not a struggle for existence or for supremacy, not a devastating scramble for the material goods of this world, but a generous and fruitful emulation in the creation and the diffusion of spiritual values. Now this creation takes place to a large extent secretly, for it is not accomplished by crowds, nor by pompous dignitaries officiating in the eyes of the people, but by individuals often poor and unknown, who carry on their sacred task in mean garrets, in wretched laboratories, or in other obscure corners scattered all over the civilized world, without hardly any regard for political boundaries, social or religious distinctions. « The wind bloweth where it listeth. » (1) The secrecy of their work is enhanced by the fact that it goes on in spite of the catastrophies, wars and revolutions which retain the whole attention of the people. Wars and revolutions are not essentially different from natural catastrophies such as earthquakes, volcanic eruptions, floods or epidemics; they are almost as impersonal and incontrollable. For most men these great catastrophies are by far the most important events, and this is natural enough, since their welfare is dreadfully affected by them. GALILEO's or NEWTON's discoveries dot not raise the price of food or shelter, at least not with sufficient suddenness to be perceptible. For us on the contrary, these discoveries which must sooner or later transform man's outlook and so-to-say magnify both the universe and himself, are the cardinal events of the world's history. All the catastrophies caused either by the untamed forces of nature or by the irrepressible folly of men, are nothing but accidents. They interrupt and upset man's essential activity but however formidable, they do not and cannot dominate it. This is summed up in what I might call our fourth principle : ***The essential history of mankind is largely secret.*** Visible history is nothing but the local scenery, the everchanging and capricious background of this invisible history which, alone, is truly œcumenical and progressive. From our point of view peoples and nations, even

(1) The Gospel according to St. John. III, 8.

as men, are not to be judged by the power or the wealth they have attained, nor by the amount of perishable goods which they produce, but only by their imperishable contributions to the whole of humanity.

II. — *The unity of knowledge.*

Many scientists and scholars fail to grasp the unity of knowledge because their innate lopsidedness or the very trend of their research has gradually driven them to restrict their attention to one very special branch of science. Little by little they loose contact with other branches and finally they become unable to understand the relation of their own studies to the whole of knowledge. This condition is particularly ominous, when they have specialized too early without having first tried to know at least the elements of the other branches. Such people are but too often prone to deny a unity, which has ceased to exist for them, and, because of their own failure to master a small department of science (1), to assert the impossibility of embracing the whole of it.

The very fact that the vast majority of scientists are excessively specialized, causes encyclopedic knowledge to become more and more needed. The very few men who attempt to compass such knowledge are the true guardians of the scientific spirit. Without their centripetal activity, the smaller branches might thrive just as well, the number of scientific facts and concepts might go on increasing indefinitely, but the tree of science would slowly wither and die. Encyclopedic knowledge is undoubtedly necessary. It is also possible, even as it is possible to draw a map of the world, based upon the analytical efforts of thousands of geographers. Of course such knowledge is not meant to replace the more detailed knowledge attained by specialists, but simply to evidence the relations of each part to every other part and to the whole, to unify and to organize. It would be absurd to expect even the best encyclopedist to know as much biology as a biologist or as much physics as a physicist, yet much of the criticism adressed to them is based upon such an unfair expectation. There is but little excuse, however, for the occurrence

(1) They naturally fail to master it because of their ignorance of other departments, because of the very narrowness of their knowledge.

of scientific mistakes in their publications, for they can and should avail themselves of the more intimate knowledge of specialists. In brief, specialists and encyclopedists, or analytic and synthetic scientists, are equally necessary; their activities are correlative and complementary. They are all of them, the servants of science. They must not let distrust and faultfinding separate them; they should rather recognize their mutual dependency and respect and help one another.

One of the fundamental problems to be solved by the encyclopedist is the classification of sciences. As soon as the number of objects to be considered in any investigation ceases to be small, the very infirmity of our mind obliges us to divide them into groups, and these, if necessary, into smaller groups, and so on. Thus he who would survey the progress of science must needs classify scientific facts and theories as naturally and logically as possible. The encyclopedist classifies concepts even as the entomologist classifies insects. The activity of the former is just as scientific as that of the latter, for a clear concept is as tangible an object as a beetle. We should not forget, however, that such classification is necessarily artificial, far more artificial indeed than our classification of plants or animals. The very evolution of science classifies already a good amount of it, for the concepts investigated by each specialist from so-to-say a natural group. However, it is soon found that these groups are not exclusive but trespass upon each other's domain in many ways. The problem is thus to choose among the various groups those which are fundamental and to subordinate the others to them as well as possible. My image of the tree helps us to realize how this can be done. The main branches which derive directly from the trunk represent the fundamental subdivisions; after that we can recognize secondary branches or branches of the second order, branches of the third order, of the fourth order, etc.

This gives one also a way of estimating the importance of a new scientific discovery. A scientific discovery is either the detection of a new fact, or the establishment of a new relation between old facts and their rearrangement. In either case it can be symbolized on our tree by a new twig or by one of the intersecting lines already described. The importance of the discovery is then easily deducted from the position of the twig : if it is placed at the periphery of the foliage it is not likely to be very significant because it will affect but a few of

the smallest branches; if, on the contrary, it is located near the center of the tree on one of the primary branches its importance is probably considerable for it may influence and transform an enormous province of science.

Each new concept introduces a new branch of science. This enables one to build up a primary classification which is, in spite or some artificiality, of great service. For example, the concept of space opens the field of geometry; the concept of number that of arithmetic and analysis. Add to those the concept of time, and we can construct mechanics. Various other concepts, less fundamental, lead to the development of the physical sciences : astronomy, physics, chemistry, etc. New fundamental concepts, such as life and mind are needed to introduce the new departments of knowledge which we call biology, psychology, sociology. The utility of that classification, which is so well known that my little sketch of it is quite sufficient, is based upon the fact that it makes possible the arrangement of the main provinces of science according to the number of fundamental concepts which form the core of each. Thus mathematics are based on only two of these fundamental concepts; mechanics on three; biology on four; psychology and sociology on at least five. This classification suggests that the relations between these provinces are not by any means equal; their dependencies are not reciprocal. This is amply confirmed by a deeper study of the subject. For example, a physicist must know mathematics, but it is not essential for a mathematician to know physics; a biologist should have some knowledge of mathematics, physics, chemistry, but a physicist may to a large extent ignore biology. It follows from this that the encyclopedist should be primarily a mathematician. If not, his synthesis can never be truly profound and complete; he can never embrace the whole tree of knowledge but must be content with a few of its primary branches. To use another illustration, we may liken the encyclopedist unto a man climbing a mountain in order to have a general view of a large country. It is clear enough that the survey of each man will depend upon the sum and quality of his knowledge, but irrespectively of that, only the mathematician will be able to reach the very top. The chemist, knowing but little or no mathematics will have to stop at some distance below; his horizon will be smaller. The biologist, ignorant of mathematics and physics, will begin to be impeded much lower still; the physician

without real scientific education, will not be able to climb at all. It is this very condition of affairs, so distressing to those who are the victims of it, which causes so many biologists and naturalists to distrust the possibility of encyclopedic knowledge. Such knowledge is indeed hopelessly beyond their own reach; they are utterly unable to see the scientific theories in their true perspective and, as they do not realize the true reason of their failure, they believe that everybody is bound to fail even as they do.

Encyclopedic knowledge is necessarily limited with regard to the innumerable details and applications of each discipline, — this does not matter inasmuch as it is not meant to replace but simply to coördinate special knowledge, — but it cannot safely be restricted as far as the theories themselves are concerned. That is, the encyclopedist must have as deep an understanding as possible of the cardinal facts, the crucial problems and the main theories of each science; if not, his knowledge is worthless and his efforts are doomed to remain sterile. He must be able to contemplate the whole fabric of science, not from below but from above, from as high a point of view as the development of knowledge permits, or else, he is utterly unfit to accomplish his task. It is necessary to insist on this, because this point, obvious as it is, is frequently misunderstood. Thus any kind of uncharted information, if it be sufficiently massive; any random accumulation of facts, even the most superficial; any sort of intellectual *olla podrida*, is apt to be mistaken for true encyclopedic knowledge (1). It is because of such misunderstandings that the latter has but too often fallen into disrepute. Encyclopedic knowledge has no more right to be superficial than any other sort of knowledge; its value is a function of its depth and solidity. Any kind of knowledge implies a selection and classification of facts; the selection and classification of the encyclopedist are not the same as those of the specialist; they are made on a different scale, but

(1) It is true, the word encyclopedia and its derivatives have lost their original meaning, because the existence of various important works, entitled Encyclopedia, reminds us of a collection of information arranged in alphabetic order. Such order is, of course, exceedingly convenient but equally artificial. Let me say, once for all, that when I speak of encyclopedic knowledge, I never mean dictionary knowledge (which is, in some ways, an extreme instance of disintegrated knowledge) but on the contrary, as the etymology suggests, organized and unified knowledge.

to have any worth they must be made with at least as much accuracy and intelligence. The aim of encyclopedic endeavors is to unify and deepen our knowledge; incidentally they simplify it, but this simplification is essentially different from that aimed at and obtained by popularizers. Encyclopedic knowledge is not by any means elementary knowledge, though it is not necessarily of exceptional difficulty. It is synthetic, organic, classified, unified and simplified knowledge.

One very valuable result of one's efforts to attain such knowledge is the keener realization of one's ignorance, or to put it otherwise, of the approximative and provisional nature, the essential relativity, of science. To be sure, the scientific knowledge already available is marvellous enough. The laws of nature are subjects for infinite wonder, but is their gradual discovery by man not even more wonderful? Among the innumerable splendors of nature, none are more sublime than those created by the human mind. However, each addition to our knowledge as it broadens our intellectual horizon, extends our contact with the mysterious unknown, the infinite darkness which is beyond. There are far more mysteries in the world for the man of science, especially for him who is of the encyclopedic type, than for the ignorant, but they are not mixed up indiscriminately with the rest of his thought; he knows where they are for he is painfully aware of the gaps and of the imperfection of his knowledge.

The attitude of the philosophical scientist of to-day is thus materially different from that of the positivist of a century ago. It is still a positivism, because constant pains are taken to separate very sharply the things that we know positively from the others and to built our philosophy primarily upon them, but it is a positivism tempered by greater humility and charity.

I feel more and more deeply that the very core of what I would call a scientific education is the insistence upon truth. The greatest number of people have no clear idea of it. The education imparted by literary people does not inculcate the value of truth, because most of their statements are of necessity purely qualitative. It is equally correct, for example, to say « this man is good », or « this man is bad », because any man is good in some respects and bad in others, and because such statements are not amenable to a rigorous verification. It is only in the laboratory — for real life is far too

complex — that one can learn the real signification of truth. Unfortunately a great number of experimenters are so specialized that their truths, however accurate within their little domain, are incomplete. The discovery of truth implies at once the critical use of good methods, a sufficient broadness of outlook and boundless generosity. But everything in life seems to conspire against the truth-loving attitude, which is the essence of the real scientific spirit. Not only does the usual literary education discourage it, but many conditions of everyday life run directly against it. For example, the Roman maxim « *caveat emptor* » is still as apposite to the commercial standards of our day as to those of two thousand years ago. A seller is not supposed to volunteer information discrediting his merchandise even if his silence is, in fact, a deceit. And what shall we say of the legal training which molds the charaters of so many of our leading men? No lawyer, no politician is expected to mention facts which do not favor his cause. In forensic practice this does perhaps not matter so much, for the judge knows that each litigant tells only one side of the story and that the truth must be somewhere between. But think of the havock which is made when such methods are transferred from the forum to the study, when only one party is heard and there is no judge! Yet such is the influence of juridical education that most men deem themselves sufficiently honest if they do not tell positive lies. They constantly distinguish between useful truths, which it is well to state, and unuseful, self-damaging truths, which it is not dishonorable to withhold; they are apparently unaware that, under such conditions, the real truth can but seldom be known. There is very little difference, indeed, between half-truths and half-lies. No wonder then that it is almost impossible to obtain accurate information on political and social affairs. Even scientists are not free from such reproaches (the war has given us repeated proofs of it), because they themselves have received no scientific education and because their outlook is often very narrow. The consequences of all this are simply frightful. For example, the combination of some ethical knowledge with this legal conception of truth is the very recipe of moral and religious hypocrisy. Many people get rid instinctively of all the facts which do not tally with their prejudices; it is as if their mind was protected by a semipermeable membrane letting the agreeable truths in and leaving the disagreeable ones out. From the scientific point of

view this is nothing but intellectual dishonesty, but they call it optimism.

The saying of NIZOLIO (1) in his *Antibarbarus* that scholasticism is the capital enemy of truth, must be understood in a similar way. For every kind of scholasticism, whether it be Christian, Jewish, Muslim, Buddhist, Vedantist or Neo-Confucianist, is the deductive development of a limited set of observations and experiments; the neat formulation of an authoritative and final doctrine. The small amount of truth upon which it is based is gradually diluted into a mass of arguments more and more remote from nature; it degenerates very soon into a rigid system, which, in spite of all the truth it may contain, is fundamentally erroneous.

These remarks on truth may seem to take us away from our subject, but they do not. The burden of this chapter, indeed, is to insist upon the necessity of encyclopedic knowledge. I have tried to show that knowledge, to be truly valuable, must be sufficiently comprehensive and congruent. Science is not simply an aggregation of isolated facts, however pregnant these may be; it is an organized concatenation of them; not a final one, but on the contrary, one constantly subject to addition and revision. The scientific spirit is essentially the love of truth, — not the selected and unverifiable truths of literary men, not the fragmentary truths of lopsided scientists, not the unchangeable truths of the scholastics — but all the truth available, whether it be useful or not, pleasant or unpleasant. Incomplete truth is no truth at all. In many cases it can hardly be distinguished from downright falsehood. The main purpose of a scientific education would be to explain the methods of reaching the truth, all the truth within one's grasp, and to inculcate the habit of needing and telling it. It would teach people not to be satisfied with less and to feel as humiliated if they withheld the unfavorable aspect of their case as if they had deliberately falsified the facts involved. Encyclopedic knowledge is the aim; critical observation and experiments, unreserved disclosure of the results, are the methods.

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(1) As quoted by RENAN in his AVERROËS (second and later editions, p. IV, 393). The Italian humanist MARIO NIZOLIO taught at Parma and later at Sabbioneta, where he died c. 1576.

III. — *The unity of mankind.*

I explained in the first chapter that unity of knowledge and unity of mankind are correlative notions. It is easy to show that the scientific organization of the world and its internationalization are also correlative; either one must necessarily drag the other in its train. For one thing, science — at least that part of it which has already become classical — is the common thought of the whole world; it is the organized body of all the facts and theories from which almost all arbitrariness has been excluded, upon which enlightened people are unanimously agreed and which is placed temporarily beyond the range of discussion. The domain of classical science is the privileged domain of internationalism, for it is already the common patrimony of all men. Moreover science constitutes the very axis of human advance and furnishes the very principles and the fundamental methods of social organization. The progress of knowledge and of intercourse introduces continually new social ties; the solidarity and interdependence of men grow almost automatically together with the multiplicity and the precision of these ties; this solidarity becomes more and more an organic need of our life. The unconscious efforts of all peaceful men, strenghtened by the conscious endeavors of a few scientists and organizers, are thus slowly preparing a higher sort of international brotherhood.

The unity of mankind is proved by their common purpose, the creation of spiritual values, mainly the development of science (1). The nobility of man is constituted by this common purpose, by this immense and endless collaboration, by the memory of past accomplishments all pointed in the same direction, by the very treasure of beauty and knowledge accumulated by his ancestors. The collaboration of all the peoples of all times is effected primarily by

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(1) The fact that the different races of men have not been equally productive is no objection to this, for they are nevertheless collaborating in the same great task. The contributions of women have been on the whole considerably smaller than those of men; yet when we speak of men, we always include women. Moreover the race which has been hitherto chiefly responsible for the development of positive science might be annihilated, — for example, if no method is found to stop internecine aberrations. The fulfilment of mankind's destiny would then devolve upon other races.

a few men of genius, who may appear almost anywhere, but also by the division of analytic labor between a great number of other men, patient and conscientious scholars, well trained observers and experimenters. Indeed the systematic exploration and description of the universe can be accomplished only by an international coöperation involving many thousands of investigators. Besides, each great discovery, each new synthesis, may necessitate a tremendous amount of fresh inquiries, which cannot be successfully carried out unless all nations do their share of them.

I must especially insist upon the collaboration of East and West, because it is but too often overlooked or misunderstood. Most historians are so dominated by occidental prejudices that they can hardly imagine any other civilization than the one resulting from the double stream of Greco-Roman and Judaeo-Christian traditions; at any rate they behave as if no other civilization were worth considering. One might grant that the greatest number of sientific facts and theories have been discovered by the Mediterranean nations and by the Christian peoples of Western Europe, yet a good many others have been revealed by other peoples — Jewish, Iranian, Muslim, Hindu, Chinese, Japanese — and there is no way of measuring the respective values of their contributions. One must needs recognize that human civilization is not exclusively occidental, not by any means. Moreover the very fact that the civilizations which developed in the Mediterranean basin and in Central and Eastern Asia were largely independent, renders their comparison extremely instructive. This comparison affords another proof of the essential unity of mankind, for the fundamental problems of East and West are the same, and the solutions, however dissimilar, have yet many points of contact (1). Finally an immense cultural progress would be brought

(1) The independent and dissimilar evolutions, East and West, joined to the necessity of confronting them, makes it practically impossible to divide the past into a certain number of historial periods as most historians do, for Eastern and Western periods do not tally. Therefore in the Critical Bibliography of *Isis*, the past is subdivided primarily in centuries, which enables (and indeed obliges) the reader to make a comparative survey of East and West for each century. This decimal subdivision is very artificial (no natural one is possible) but very convenient, because a century covering about three or four generations, represents the maximal range of direct personal influence. The century period is neither too long nor too short; it harmonizes well with the length of human life.

to pass if it were possible to combine the highest ideals of East and West. We have as much to learn from them as they from us, but we will never learn anything unless we approach one another with open hearts and true humility (1).

The sympathetic study and the understanding of the thoughts and ways of other peoples is a vital necessity for every man. If he neglects it, he soon becomes provincial and looses his sense of proportion. Such necessity is nowhere more urgent than in the newer civilized countries of the world, such as the American Republics or the British Dominions. The civilization of these countries is indeed incomparably less independent from that of Europe, than the Roman civilization was from the Greek, or the Japanese from the Chinese. When I hear the flippant criticisms of Europe into which some Americans like to indulge, I cannot help thinking of children insulting their mother.

The true internationalism toward which the unity of knowledge and the unity of mankind are steadily driving us, will constitute an immense progress. This progress will be largely due to the development of positive knowledge and scientific methods. It is hardly necessary to add that this internationalism, expressing the common mission of mankind, is essentially different from that futile and sterile cosmopolitism, which would ignore or belittle national characteristics. It does not in the least attempt to supersede the smaller groups of humanity but strives rather to complete and strengthen them by a supreme organization embracing them all. The slow constitution of nations and the development of that noble loyalty of every citizen to his mother country did not destroy the family unit but on the contrary fostered and protected it, by putting some new vigor and sanctity into it. The only thing which the higher duty of patriotism did destroy was the fiendish hostility of rival families. Their differences were merged into a larger union and assuaged by a common loyalty to the whole country. In the same way international ideals do not aim at the destruction of any national or racial quality but simply at the dissolution of international

(1) A very valuable attempt to compare the philosophical developments of Europe, India and China has been made recently by Paul. Masson-Oursel. *La philosophie comparée*. Paris 1923 (reviewed below).

hatreds. National loyalties must be gradually harmonized into a higher loyalty to the common purpose of mankind.

It is important to observe that loyalty to truth would almost suffice to realize this immense step forward. If everybody placed the love of truth and justice even above the love of his family or of his country, there would be no reason to fear the disintegration of humanity by a new civil war. Nor could any country be the loser by this, for what could it hope to win by falsehood or injustice? Even as no honest man would care to obtain advantages for himself or his family by misrepresentation, no honest country would attempt to magnify itself by force or fraud at the expense of other countries. If the truth standard of politicians and diplomatists was the scientific standard instead of the legal or commercial, our international ideal would be accomplished without any difficulty.

In the meanwhile, whatever happens at the surface, the essential unity of mankind is a reality which will be attested in the future as it has been in the past by a small elite of scientists. These will pursue their sacred task, undisturbed — even as bees pursue theirs — in spite of wars and other cataclysms. Thus, thanks to them, the secret advance of mankind may go on indefinitely.

IV. — *The history of science.*

From the technical point of view, the point of view of everyday life, no knowledge is worth considering but the very latest. From the philosophical point of view, on the contrary, the consideration of the latest stage of knowledge is absolutely insufficient. Because of the essential relativity and incompleteness of science we cannot fully understand it unless we see it flow and develop. Science is not a being but a becoming. Fixed knowledge is dead knowledge; we are interested primarily in live knowledge. We are very anxious to get acquainted with the latest theories, but that alone cannot possibly satisfy our curiosity; we must know their ancestry as well, we must be able to retrace their evolution from the very beginnings until their present state. Thus it is indispensable to study the history of science.

You remember my representation of knowledge by a tree. This helped us to conceive the possibility of a natural classification of sciences. In the case of living beings too, a classification to be

adequate must tally with the facts of family relationship and descent. These facts are established mainly by paleontological and embryological, that is, by historical methods. Even so, an adequate classification of sciences involves historical investigations. The only safe way of unravelling it, is to study carefully the development of the different branches of science from the earliest times step by step until to-day. Purely deductive classifications, which do not take the facts of ancient science into account, are of no value whatsoever. Similar investigations are necessary to solve satisfactorily every scientific problem which is not restricted to utilitarian and immediate applications.

On the other hand, from what I have said in the previous chapter of the scientific nature of the internationalization of the world, it follows that it is not possible to study the development of human organization (not simply the organization of knowledge but of society) without continual reference to the history of science. The history of science is, indeed, the skeleton of the history of civilization. In particular, those who would understand the advance of internationalism and further it, must needs study the history of science; there is, in my opinion, no better way of giving to their activity the philosophical foundation which it lacks but too often. The sublime recital of man's intellectual progress throughout the ages, would fire their enthusiasm, stimulate their devotion, increase the convergence and the efficiency of their endeavors.

Whether our interest be primarily philosophical or sociological, as soon as we realize that our knowledge of nature and of man cannot be complete or sufficient unless we combine historical with scientific information, the history of science becomes so-to-say the keystone of the whole structure of education.

Unfortunately this is not generally understood either by historians or by scientists. Their lack of understanding, however painful, is not surprising. It is clear that historians who know nothing of science or scientists whose conception of historical methods is of the crudest kind, cannot appreciate the expediency or even the possibility of harmonizing their respective points of view. Either group is contemptuous of the other and deems its own task self-sufficient. Although some change towards a wiser attitude is already perceptible, the great majority of historians are still unaware of the very gravity of their ignorance. It has not yet dawned upon them that only such men who

can really grasp the meaning of human progress and can analyse its internal structure are truly qualified to explain the past. The attitude of most scientists does not prove much more enlightenment. Many of them will go out their way to express their contempt of historical work, thus revealing their deep ignorance of it. They believe apparently that history is still in its literary stage : they do not realize that the historical methods of to-day deserve to be called scientific to the same extent as many methods used by naturalists; they forget also that the methods which they themselves are using have not always been as rational and rigorous as they now are. The methods of every discipline improve gradually; the organization of some sciences grows faster than that of others but they all have very humble beginnings. It may be objected that historical research leaves no room for experimentation, but the same can be said of many branches of natural history; there remains in every case ample opportunity for critical observation and comparison. The main difficulty is caused by the uniqueness of historical facts; they happen once and are never exactly repeated. Yet comparisons are still possible between facts of a similar kind, and there is no danger in this if full allowance is made for every dissimilarity. Granted that historical knowledge is of its nature less accurate than that obtainable in other descriptive sciences, it would be very foolish to despise it on that account. The aims of the scientist and of the scholar are identical : to reach the truth — positive, objective truth — as much of it as can be attained. The smallness of one's knowledge is no disgrace if no more be within one's reach and if one remain keenly aware of its limitation.

Many men of science lack historical spirit, that is, they are not able to see the past in its true perspective. They will say, for example, that the development of science before the XIX^th^ century, or before the XVI^th^, is not worth knowing because it is too rudimentary. They forget that to compare medieval science with our own, is absurd and injust. We have succeeded in reaching the present level because we stand on the shoulders of people who had reached a lower level before; if these people had not done their part of the ascent, others or we ourselves would have had to do it. To pass sentence over early science from the point of view of our own is as unfair as to refer children actions to grownups standards. To appreciate justly previous stages in the development of science,

we must not compare the knowledge of a certain period with our own but rather with that of the preceding period; we must ask ourselves whether these people knew more or less than their fathers, whether their endeavors, no matter how modest, were made in the right direction. Our present knowledge will have to be judged some time in the same way (1).

The main reason of the lack of interest in the history of science is the separation of the intellectual elite into two rival groups. This separation itself is largely caused and at any rate aggravated, by the fact that education has not kept pace with knowledge. In spite of the tremendous progress of science, a truly scientific education is not yet organized anywhere.

To come back once more to the tree of knowledge, the studies which I advocate in order to reconcile the historical with the scientific spirit, would be of two different kinds. One may investigate the state of knowledge and civilization at a definite period, that is, one may study transversal sections of the tree. The whole history of science might be conceived as a complete series of such sections. Or else, one may study the development of a branch or group of branches from its roots to its youngest twigs. This corresponds to the history of particular sciences. Studies of the second kind are fairly numerous, especially with regard to mathematics, mechanics, astronomy, chemistry, geology and medicine. Studies of the first kind, though their cultural value is considerably greater, are on the contrary very rare. One might say that knowledge and

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(1) MACAULAY expressed similar views in his essay on Sir JAMES MACKINTOSH's History of the Revolution in England in 1668, London 1834 : «As we would have our descendants judge us, so ought we to judge our fathers. In order to form a correct estimate of their merits, we ought to place ourselves in their situation, to put out of our minds for a time, all that knowledge which they, however eager in the pursuit of truth, could not have, and which we, however negligent we may have been, could not help having. It was not merely difficult but absolutely impossible, for the best and greatest of men, two hundred years ago, to be what a very commonplace person in our days may easily be, and indeed must necessarily be. But it is too much that the benefactors of mankind, after having been reviled by the dunces of their own generation for going too far, should be reviled by the dunces of the next generation for not going far enough. »

civilization have not yet been completely and carefully mapped for any period, excepting perhaps the golden age of Greece (1).

To give a clearer idea of the extent of our studies, I will indicate two other historical surveys which would complete them : the history of art and the history of religion. But it should be noted that to parallel the history of science, as I understand it, the history of religion should include all religions and the history of art, all the fine and applied arts and the belles-lettres of every nation. Even as the historian of science explains the progress of positive knowledge and of its applications, the historian of religion would explain the development and purification of beliefs, the historian of art, the elaboration of dreams. The three histories put together would constitute a perfect mirror of the gradual expansion of man's thought. The history of science would be, of course, the central history, for it would be, among these three, the only one evidencing a continual accumulation and improvement. In spite of a few momentary regressions, the history of science is, indeed, essentially a tale of progress, of conquest; the progress is slow but sure, the conquests are inalienable; man cannot tell another tale of such greatness. It is unique. This is especially obvious if the history of thought is truly encyclopedic and œcumenic, for peoples or races may degenerate or disappear, or some branches of science may be temporarily neglected, but if one takes a broad view of the whole tree of knowledge, deriving its substance from the whole world, the growth may be sometimes irregular, it is never interrupted.

V. — *The New Humanism.*

The greatest difference between the history of science, as I understand it, and general history, as most historians understand it, is that the latter does not allow us to draw any definite conclusions, while the former leads directly to a new philosophy. The history of science cannot be an end in itself, but a means to a higher end : a deeper understanding of science, of nature, of life. It is possible that there is no logic in the course of political events and that it is vain to speculate on the calamities of the past, but there is certainly a logic, a very rigorous one, in the development of knowledge, — for this

(1) Even then contemporary achievements of other peoples have been neglected.

development is progressive and cumulative. General history is largely a history of passions — the passions of single men or larger groups — and have human passions varied materially in the course of time? It is true, it includes also the history of institutions. This part of it is considerably more instructive and forms a sort of transition to our studies, for the development of institutions is obviously a function of the progress of knowledge.

The philosophy to which the history of science leads is what I have called the *New Humanism*. It embodies all the ideas set forth in the previous chapters; its principles are the very principles which I explained at the beginning. The unity and the organization of science are evidenced by the study of its history. The same study explains and proves the progress and the unity of mankind.

The name of this philosophy is justified by the fact that it implies a humanization of science, a combination of the scientific and humanistic spirit. I call it New Humanism with reference to the older humanism of the Renaissance to which it cannot improperly be compared. The old humanism was a revival of the ancient knowledge and wisdom fallen into neglect during the Middle Ages, its integration with the rest of contemporary culture; it drew its strength and inspiration from the vast treasure of Greek and Latin literature. In the same way, the New Humanism is a revival of the knowledge patiently elaborated and accumulated for many centuries by men of science, but neglected and despised by men of letters and educators, — its integration with the rest of our culture; its main spring is the history of science. It undertakes to bring together for the first time, scientists, historians, philosophers, sociologists; to coördinate and harmonize their points of view; to broaden their horizon without lessening the accuracy of their thought; to make the accomplishment of their higher task easier in spite of the increasing wealth of knowledge.

I said that the Renaissance was a resurrection of ancient wisdom. This statement is correct but needs qualification. The humanists and even more the contemporary men of science who turned to antiquity for fresh inspiration, did so as freemen, having a will of their own. Their reverence for the past was deep but critical. The main characteristic of the Renaissance, indeed, was its individualism, its refusal to submit blindly to authorities. It was essentially anti-scholastic. Even so, the New Humanism recognizes scholasticism as its arch-enemy.

Such movement is becoming more and more necessary for many reasons. In the first place, the intellectual laziness of most men, the ignorance and pedantism of educators, a load of knowledge, growing beyond the power of endurance of many, ancestral terrors and superstitions ever lurking, all these circumstances conspire to revive or to fortify scholastic tendencies. Then, as I have said above (but the matter bears repetition), the separation of the elite into groups unable to understand one another is ominous. It jeopardizes at once the unity of science and the unity of mankind. Men without scientific knowledge are totally unfit to explain the progress of humanity. Uneducated scientists, — men with short memories and no sense of perspective — are bound to be lopsided, narrow-minded, unimaginative: they are constitutionally unable to have a clear and deep understanding of science, not to speak of history. The best fruits of their own studies are lost to them. Investigations carried on along very special lines without reference to the work of one's fellowmen must produce in the end a sort of scientific Pharisaism, a worthless and stupid idolatry of facts, a system of meaningless conventions and unconscious prejudices. All these dangers are considerably increased by the fact that, as science is developing all the time, more and more of it will pervade our life (whether we like it or not) and eventually, when the inertia of schoolmen is overcome, our education. Science is our greatest treasure but it must needs be humanized or it will do more harm than good.

The New Humanism is a compromise between idealism and knowledge. I do not know which is worse, idealism without knowledge or knowledge without idealism, yet in the present circumstances we are but too often obliged to choose between the two evils. We need ideals to guide our steps toward the future, but they cannot possibly be fulfilled if we are not aware of the obstacles to overcome, of the paths to be followed, of the dangers that lie in them. Even the ideals themselves cannot be clearly conceived if one is not sufficiently familiar with all the knowledge available in one's time. Nothing can be more demoralizing than unreasonable ideals.

We might say also that the humanization of science is a compromise between the claims of the future and those of the past. Many are the poets and philosophers, who, realizing deeply the evils caused by the very progress of civilization, have dreamed of reëstablishing the purity, the simplicity and other idealized condi-

tions of former times. But that is a false ideal or one which is wrongly formulated. We must interrogate the past in order to obtain a better understanding of our sacred patrimony, of our present position, of ourselves; we must contemplate it for the sake of its beauty and nobility; yea we must revere it, but a return to it such as RUSKIN, TOLSTOI, TAGORE and others have preached, is truly impossible. Humanity cannot go backwards any more than a grown man can recover the innocence and carelessness of his childhood.

Knowledge is one but its sources are many. Follow them and you will find that they can be reduced finally to two : nature and history. Either is necessary; neither is sufficient. The New Humanism is an emphatical recognition of that fact. Scientific and historical knowledge are equally indispensable; neither can completely enlighten us without the other. The New Humanism is an endeavor to preserve the best traditions of the past, to integrate them with our newer discoveries; to temper our knowledge with grateful memories and, so-to-say, to atone for the intellectual progress which it is our destiny to accomplish (sometimes faster it seems, than we deserve) by an increased devotion and more humility. The scientists of to-day whose privilege it is to contemplate the whole past and to dominate nature from the summit of modern science, must not be unworthy of their exalted position. No phase of human experience must be foreign to them; they must redeem their knowledge and the tremendous power it confers by humanity and love. They it is who will transmit the sacred treasures of our ancestors to our children. They must look back with gratitude and veneration towards the past, yet go forwards. Whatever salvation there be, is forwards not backwards. The past, however beautiful, is less beautiful than the future... The New Humanism derives its main inspiration from the past, yet it is turned towards the future.

George SARTON.

Intervale, New Hampshire.
September, 1923.

APPENDIX I.

The publication of « Isis ».
(An urgent appeal to American scholars.)

I started the publication of *Isis*, ten years ago, in 1912-1913, to collect materials for the history of science and to explain and promote the very ideals to which this essay is devoted. I had then but little experience and my knowledge was considerably smaller than it is now. Without the encouragement of many of the leading men of science of the world (1), I would never have dared so bold an undertaking. By the middle of 1914, when the war broke out, *Isis* was fairly well launched. When I resumed the publication in 1919, after many tribulations, many of the former subscribers could not be reached any more : some were dead, others were ruined, others still had turned to other subjects. The work of propaganda was to be done all over again. Yet the European market was largely destroyed and I was myself impoverished. My income was now almost exclusively derived from my labor and, owing to the very high cost of living and to various accidents, I was obliged to live from hand to mouth. In spite of the devoted collaboration of my wife, I could not improve my condition. Since 1919, I have spent, on the average, twenty per cent of my salary for the publication of *Isis* and for my studies.

It should be noted that the effort which I am making to promote the History of Science and the New Humanism, is unique, for *Isis* is essentially different from other reviews of general science, such as *Nature*, *Scientia*, *Science progress*, *Revue générale des Sciences*, *Die Naturwissenschaften* or *Science*, whose aim is scientific (and professionnal) rather than humanistic. *Isis* is perhaps nearer to *Scientia* than to any other review, yet it is very different from it, because it does not contain the many valuable essays on modern theories which are found in *Scientia*, but does contain a vast amount of information on the history of science which cannot be found

(1) Especially of the French mathematician Henri Poincaré and of the Belgian sociologist Emile Waxweiler. — both gone alas ! — Thanks to the latter I obtained some financial help from the chemist Ernest Solvay for my start.

anywhere else. *Scientia* is a review of high popularization which appeals naturally to a larger public; *Isis* is, on the contrary, a review of scientific research edited primarily to satisfy the needs of a special group of scholars. Other reviews devoted to the history of science are published in Germany (the *Mitteilungen zur Geschichte der Medizin und der Naturwissenschaften*; the *Archiv für die Geschiche der Naturwissenschaften und der Technik*) and in Italy (the *Rivista di storia delle scienze mediche e naturali*, and Aldo Mieli's excellent *Archivio di storia della scienza*), but none of these reviews is as comprehensive as *Isis*, and none is as deeply concerned with the philosophical and sociological sides of the subject, the ideals of the New Humanism.

The first five volumes of *Isis* (1) contain 70 papers (2), 527 reviews and some 9,196 bibliographic notes. The reader may be reasonably sure to find in them a critique, a brief analysis or, at the very least, a mention of every publication of any importance relative to any aspect of the history of science and subsidiarily of its philosophy and of the history of civilization.

The main papers are generally not meant to be read (except a few editorials) but to be studied. The aim of *Isis* is not by any means to entertain idle readers but to collect and publish the best information available, to coördinate the work of scholars scattered in many distant places, in brief, to organize the history of science as an independent and full-fledged discipline placed on the same level as, say, the history of art or the history of religion. For example, the editor is anxious to publish original scientific texts, that is, to provide material for later investigations. The publication of Oriental texts (or rather of translations of them) is especially important, for a large proportion of them are still unpublished and virtually unknown. The subsequent volumes of *Isis* will contain — in shâ Allâh — many translations of Arabic, Persian, Sanscrit, Chinese and Japanese texts.

Most of the original papers, I said, are not meant to be read; the reviews, on the contrary, should be read, as much as possible, by all students of our subject and the more so that these students

(1) 3118 pages, that is, an average of 624 p. per volume.

(2) The papers are written mainly in English and French, but also in German, Italian, and Latin.

will find in them, at least in some of them, many remarks of methodological value. However the most useful part of the journal is probably the Critical Bibliography which is not paralleled in any other publication. The material collected in it and duly classified is not restricted to the history of science; some of its deals with the philosophy of science or with philosophical problems which may confront the scientist; another part deals with the history of civilization or with the history of art or the history of religion. In short, everything which may help the historian of science to obtain the information which he strictly needs, is included, but beyond that, the editor tries to give him every inducement to keep a generous view of his studies and to be, what he might and should be, a true humanist.

The classification is primarily chronological, by centuries (1). Material which cannot be classified in that way is then if possible, classified historically (2), and if that fails, systematically (3). The purpose is to oblige the reader to consider the development of mankind — not simply one province but the whole of it — in its true chronological sequence. He cannot possibly use my classification without being constantly induced to make comparisons between the achievements of various peoples of East and West. Neither can he use it at all without realizing that many branches of the tree of knowledge grew together, each one influencing the others. Thus this classification offers, so-to-say, a continual illustration of the unity of mankind and of the unity of science.

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(1) See in chapter III, the footnote justifying the centurial classification.

(2) Under the following headings : Antiquity; Asia; Babylonia and Assyria; Byzantium; China; Egypt; Greece; India; Iran; Islam; Israel; Japan; Middle Ages; Rome.

(3) Under the following headings, briefly indicated : Anatomy; Anthropology; Archæology, museums and collections; Art, art and science, iconography; Arts and crafts; Astronomy, geodesy, meteorology and terrestrial physics; Bibliography and libraries; Biology; Botany; Chemistry; Economics; Education; Ethnology, primitive and popular science; Geography; Geology, mineralogy, palæontology; History of civilization; Language and literature; Logic and theory of knowledge; Mathematics; Mechanics; Medicine; Morals, moral organization of society; Pharmacy; Philosophy; Physics; Physiology; Prehistory; Psychology; Religion, science and religion; Science (bibliography, history, organization, philosophy); Sociology, jurisprudence and positive polity; Statistics; Superstitions and occultism; Technology; Zoology; Alia.

The editor takes special pains to publish careful reviews of all the works of real importance, — the scholarly works which are the fruit of first hand investigations as distinguished from the books which are written and produced mainly for commercial reasons. It is his ambition to provide a scholarly and intelligent critique of every scholarly work; he should not be too severely blamed if he has not yet succeeded more completely in doing it. It is certain that nothing would promote our special learning more than the publication of a careful review of each important work by the best judge. It has been regretted that it took so long before a review appeared in *Isis*. A speedier publication is certainly very desirable but the editor's very small resources restrict his freedom. Moreover accurateness and fulness are the aim, rather than speed. A delay of a year or two is of little concern for a work of lasting value and it is hardly worth while to review others.

If the editor succeeds in obtaining sufficient assistance, he proposes to increase the number of illustrations — portraits, facsimiles, maps, diagrams; he has not the means of doing it himself.

This introduces the financial question, the appeal to the reader. If the latter does not like to be « touched », it is high time for him to run away. To put it as briefly as possible, I cannot and do not expect help from those who do not understand my point of view and my aims. But am I not justified in expecting those who understand me, who use *Isis*, or if they do not use it themselves, who appreciate its usefulness, who watch me fight alone, — to come and join and help me?

How can one help? The simplest way is to subscribe to *Isis* either directly or through one's bookseller (1). Those who can afford it may order many copies to be distributed either by themselves or by the publishers according to their instructions. For example, they may order the mailing of *Isis* to the university libraries of countries whose money is debased. They can also suscribe to the advertising fund or pay directly advertisements in other journals.

Scholars who are blessed with more genius and good will than worldly goods can also help in many ways. The Critical Bibliography to be truly satisfactory requires the collaboration of a great number

(1) The simplest way is to join the History of Science Society. See preface to this volume. (Note added at the last moment. G. S.)

of specialists. I would be especially grateful to qualified scholars who would undertake to sub-edit one of the chapters, *e. g.*, Greece, or Middle Ages, or astronomy, or botany.

At any rate, I do hope, that the readers who apreciate the importance of the History of Science and who believe in the New Humanism, will not let me fight alone but will subscribe to *Isis*, even if they be as poor as its editor!

One more word. The name Isis has puzzled many persons. I chose it ten years ago because it was short, as I might have chosen Minerva, Athene, Hermes or Clio if those names had not been preëmpted. The title of a review should be as short as possible, being explained, if necessary, by a sub-title of any length. The name Isis evokes in my mind the period of human civilization which is perhaps the most impressive of all, — its beginning. Has mankind ever made as great a stride forward, as when it invented the making of fire or the rudiments of language?... However I discovered too late that this same name evoked in other minds ideas of mysteries and occultism very remote from my own. I understand that some people have been attracted to *Isis* because of such ideas (they must have been very disappointed!) while others have been equally repelled. I heard of a prominent scientist declaring that a journal called *Isis* could but be superficial and that he would not even look at it. He did not realize that it would have been difficult to surpass his own superficiality!

Gentle reader, if you are interested in the History of Science and Civilization, if you believe in the New Humanism, subscribe to *Isis* (1).

G. S.

Cambridge, Mass.
October, 1923.

APPENDIX II

List of the first friends of « Isis ».

This list of the first friends of *Isis* may help to define its aims. We often judge an undertaking by the men who joined in it. The

(1) Bona fide inhabitants of countries whose currency is temporarily depreciated may obtain special terms by applying to the Secretary of *Isis*, Mr. L. Guinet, 30, Avenue H. de Brouckère, Auderghem, Bruxelles, Belgium.

list includes all the men who contributed to any one of the first five volumes of *Isis* and those who agreed to support it at the very beginning and whose names appeared on the covers of the early numbers. Some of the latter did not contribute any papers. However, I could not omit their names because this would have involved omitting those of the much lamented HENRI POINCARÉ and EMILE WAXWEILER, to whom *Isis* is so deeply beholden. The names of those early patrons who did not collaborate have been asterisked. The names of the friends who died are followed by their dates of birth and death. It is shocking to realize how many have already left us. The names of the contributors of original papers have been printed in boldface.

Agnes Arber, hist. of botany, Cambridge, England.
*SVANTE ARRHENIUS, physico-chemist, Stockholm, Sweden.
*HENRI BERR, historian, Paris, France.
Ernst Bloch, hist. of chemistry, Brünn, Czechoslovakia.
John David Bond, hist. of mathematics, Knoxville, Tenn., U. S.
Karl Bopp, hist. of mathematics, Heidelberg, Germany.
VERA BOUNY, mathematician, Mons, Belgium.
Pierre Boutroux (1880-1922), hist. of mathematics, Paris, France.
FREDERICK E. BRASCH, hist. of American science, Washington, D. C.
C. BURALI-FORTI, mathematician, Torino, Italy.
Florian Cajori, hist. of mathematics, Berkeley, Calif., U. S.
*MORITZ CANTOR (1829-1920), hist. of mathematics, Heidelberg, Germany.
YUEN R. CHAO, Sinologue, Cambridge, Mass., U. S.
J. M. Child, hist. of mathematics, Manchester, England.
A. K. COOMARASWAMY, hist. of Hindu culture, Boston, Mass., U. S.
*FRANZ CUMONT, hist. of antiquity, Rome, Italy.
Waldemar Deonna, archaeologist, Geneva, Switzerland.
J. L. E. DREYER, hist. of astronomy, Oxford, England.
*EMILE DURKHEIM (1858-1917), sociologist, Paris, France.
GEORGE ENGERRAND, archaeologist, Austin, Texas, U. S.
Antonio Favaro (1847-1922), hist. of physics, Padova, Italy.
*FRANZ M. FELDHAUS, hist. of technology, Berlin, Germany.
*JOHN FERGUSON (1837-1916), hist. of chemistry, Glasgow, Scotland.
*ARNOLD VAN GENNEP, ethnologist, Neuchatel, Switzerland.
Aug. Georges-Berthier (1888-1914), hist. of biology, Lyon, France.
Edmond Goblot, philosopher, Lyon, France.

Icilio Guareschi (1847-1918), hist. of chemistry, Torino, Italy.
Léon Guinet, hist. of science, Brussels, Belgium.
*Siegmund Günther (1848-1923), hist. of science, Munich, Germany.
Charles H. Haskins, historian, Cambridge, Mass., U. S.
*Sir Thomas L. Heath, hist. of Greek math., London, England.
*J. L. Heiberg, hist. of ancient science, Copenhagen, Denmark.
Halldór Hermannsson, hist. of Iceland, Ithaca, N. Y., U. S.
Arthur John Hopkins, hist. of chemistry, Amherst, Mass., U. S.
*Frederic Houssay (1860-1920), zoologist, hist. of biology, Paris, France.
J. W. S. Johnsson, hist. of medicine, Copenhagen, Denmark.
Laura Jourdain, London, England.
Philip E. B. Jourdain (1879-1919), hist. of mathematics and mechanics, Cambridge, England.
L. C. Karpinski, hist. of mathematics, Ann Arbor, Mich., U. S.
G. R. Kaye, hist. of Hindu math., Delhi, India.
Graf Carl von Klinckowstroem, hist. of technology, Munich, Germany.
A. Korn, physicist, Berlin, Germany.
E. Kremers, hist. of pharmacy, Madison, Wis., U. S.
*Karl Lamprecht (1856-1915), historian, Leipzig, Germany.
Berthold Laufer, Sinologue, Chicago, Ill., U. S.
Raymond Lenoir, philosopher, Paris, France.
Walter Libby, hist. of science, Pittsburgh, U. S.
*Jacques Loeb (1859-1924), biologist, New York, U. S.
Gino Loria, hist. of mathematics, Genova, Italy.
Duncan Black Macdonald, Arabist, Hartford, Conn., U. S.
Silvio Magrini, physicist, Bologna, Italy.
Roberto Marcolongo, mathematician, Naples, Italy.
Jean Mascart, hist. of mathematics and astronomy, Lyon, France.
Paul Masson-Oursel, Sanskritist and Sinologue, Paris, France.
*Walther May, hist. of biology, Karlsruhe, Germany.
Hélène Metzger, hist. of chemistry, Paris, France.
Aldo Mieli, hist. of science, Rome, Italy.
Yoshio Mikami, hist. of Japanese science, Tokyo, Japan.
Gaston Milhaud (1858-1918), hist. of science, Paris, France.
G. A. Miller, hist. of mathematics, Urbana, Ill.
*Max Neuburger, hist. of medicine, Vienna, Austria.
Wilhelm Ostwald, hist. of chemistry, Grossbothen, Germany.
*Henri Poincaré (1854-1912), mathematician, Paris, France.

Em. Radl, hist. of biology, Prag, Czechoslovakia.
*Sir WILLIAM RAMSAY (1852-1916), chemist, hist. of chemistry, London, England.
Praphulla Chandra Ray, hist. of Hindu chem., Calcutta, India.
*ABEL REY, hist. of science, Paris, France.
ARNOLD REYMOND, philosopher, Neuchatel, Switzerland.
D. S. ROBERTSON, Cambridge, England.
Julius Ruska, hist. of Muslim science, Heidelberg, Germany.
E. S. RUSSELL, hist. of biology, London, England.
George Sarton, hist. of science, Cambridge, Mass., U. S.
LÉOPOLD DE SAUSSURE, hist. of Chinese astron., Geneva, Switzerland.
Carl Schoy, hist. of Muslim astron., Essen, Germany.
H. M. SHEFFER, logician, Cambridge, Mass., U. S.
Charles Singer, hist. of biology and medicine, London, England.
David Eugene Smith, hist. of mathematics, New York, U. S.
*LUDWIG STEIN, philosopher, Berlin, Germany.
J. Stephenson, hist. of Muslim science, Edinburgh, Scotland.
EDWARD STREETER, hist. of medicine, Boston, U. S.
Luigi Suali, hist. of Hindu phil., Pavia, Italy.
*KARL SUDHOFF, hist. of medicine, Leipzig, Germany.
LYNN THORNDIKE, Mediaevalist, Cleveland, O., U. S.
Emile Turrière, mathematician, Montpellier, France.
*EMILE WAXWEILER (1867-1916), sociologist, Brussels, Belgium.
Catherine Durning Whetham, hist. of science, Cambridge, England.
Wm. Cecil Dampier Whetham, hist. of science, Cambridge, England.
HEINRICH WIELEITNER, hist. of mathematics, Augsburg, Germany,
John Kirtland Wright, hist. of geography, New York, U. S.
*H. G. ZEUTHEN (1839-1920), hist. of mathematics, Copenhagen, Denmark.

Notes and Correspondence

Science and Religion. — Robert A. Millikan (1), director of the Norman Bridge Laboratory of the California Institute of Technology, formulated and secured the signatures to the following statement (2) :

« We, the undersigned, deeply regret that in recent controversies there has been a tendency to present science and religion as irreconciliable and antagonistic domains of thought, for in fact they meet distinct human needs, and in the rounding out of human life they supplement rather than displace or oppose each other.

The purpose of science is to develop, without prejudice or preconception of any kind, a knowledge of the facts, the laws and the processes of nature. The even more important task of religion, on the other hand, is to develop the consciences, the ideals and the aspirations of mankind. Each of these two activities represents a deep and vital function of the soul of man, and both are necessary for the life, the progress and the happiness of the human race.

It is a sublime conception of God which is furnished by science, and one wholly consonant with the highest ideals of religion, when it represents Him as revealing Himself through countless ages in the development of the earth as an abode for man and in the age-long inbreathing of life into its constituent matter, culminating in man with his spiritual nature and all his God-like powers. »

The statement was signed by religious leaders, by scientists and by men of affairs. Among the latter I notice the names of Herbert Hoover and Henry S. Pritchett. The scientists, who signed it, are : Charles D. Walcott, Henry Fairfield Osborn, Edwin Grant Conklin, James Rowland Angell, John Merle Coulter, Michael I. Pupin, William James Mayo, George David Birkhoff, Arthur A.

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(1) See *Isis*, IV, 324.

(2) It first appeared in *Science*, vol. 57, 630. June 1, 1923.

NOYES, WILLIAM WALLACE CAMPBELL, JOHN J. CARTY, WILLIAM HENRY WELCH, JOHN C. MERRIAM, GANO DUNN.

G. S.

Index cards for « Isis ». — It may interest readers of *Isis*, and even more contributors, to know that the JOHN CRERAR Library of Chicago is indexing the main papers, appearing in *Isis*, on printed cards. Vols. I and II have been systematically treated, 24 titles being printed; I understand that Vols. III to V will be treated in the same way some time this winter (1923-1924) (1).

These cards are of the size universally used in every American library, 7.5 × 12.5 cm. (a size almost equivalent to 3 × 5 i., but smaller). The JOHN CRERAR Library (Michigan Aven. and Randolph Str., Chicago) is ready to supply them at any time at 3 cents for the first card of any title and 1 cent for each additional one (bearing same title).

The cards of the JOHN CRERAR Library are found in most American university libraries, certainly in all the larger ones, and thus original papers appearing in *Isis* are brought automatically to the attention of most American scholars.

Some cards relative to the papers included in *Isis* had also been printed and distributed by the Library of Congress, but not in a systematical way. The Library of Congress cards refer to reprints from *Isis*, they are not « analyticals ». There is no reason for duplicating the effort of the JOHN CRERAR Library and *Isis* is more within its field than within the field of the Library of Congress. The JOHN CRERAR Library, indeed, is devoted exclusively to scientific subjects. The readers of *Isis* will remember that it published in 1911 a valuable List of Books on the History of Science (297 p.), followed in 1917 by a Supplement (139 p.), both edited by AKSEL G. S. JOSEPHSON. The interest of that library in *Isis* is thus natural enough.

G. S.

Collaboration with Germany and with other countries whose currency is debased. — The currency of Germany, of Russia and of a few smaller countries is debased to such an extent that the people of those countries, and even their main libraries, are unable to buy

(1) Letter from Miss GERTRUDE FORSTALL, Acting cataloguer, dated August 20, 1923

publications appearing abroad. Some twelve copies of *Isis* have been distributed to German scholars at the editor's expense. A whole set of *Isis* has been sent recently to a German address thanks to the generosity of a Parisian friend. Friends of Germany (or Russia) who happen to be also the friends of *Isis* can do a good turn to both by ordering copies of the journal to be sent to German (or Russian) libraries or to private addresses of their own choice.

The editor will be glad to increase the number of copies distributed freely in those countries. He is willing to make himself further sacrifices to that effect. But he objects very strongly to the misguided attitude of some German scholars or societies, asking free copies of our journal and then, not simply refusing to send copies of their own publications on the history of science, but charging for them an absurd price (1). Good will must be reciprocal.

G. S.

Les Tables annuelles de constantes et données numériques de chimie, de physique et de technologie. — A trois reprises déjà (*Isis*, I, 259-260, 498-500, V, 565), SARTON a attiré l'attention des travailleurs sur l'intérêt pratique immédiat de ces documents, actuellement publiés sous le patronage du *Conseil International de Recherches* et de l'*Union Internationale de chimie pure et appliquée*, et dont le secrétariat est toujours assuré par M. CH. MARIE, rue de Bagneux, 9, Paris (VI[e]). On pouvait craindre que la guerre ne portât un coup mortel à une telle entreprise; il n'en a heureusement rien été. Mais la publication a perdu son caractère annuel : les documents des années 1913 à 1916 forment le vol. IV de la publication (*Isis*, V, 565) qu'il a fallu, malgré le ralentissement de la production scientifique pendant cette période, diviser en deux parties, respectivement de XXXII+626 pages et XXXVI+752 pages. Les documents des années 1917 à 1922 seront vraisemblablement publiés, eux aussi, en deux parties, dès 1923. — De pareils volumes sont d'une acquisition difficile; aussi le Comité, prenant en considération les demandes des spécialistes, publie-t-il séparément, en même temps que le volume, et depuis le vol. III (1914), les fascicules suivants : spectroscopie, — radioactivité, atomistique, ionisation, — électricité, magnétisme, électrochimie, — cristallographie, minéralogie, — art de l'ingénieur et métallurgie, — biologie.

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(1) That is, a price which would be absurdly high even if those publications had been printed in America !

D'après un historique provisoire non publié et que m'a communiqué CH. MARIE, la collecte des documents dans les divers pays se fait par les soins de collaborateurs spéciaux (extracteurs), chargés de l'examen de toutes les publications scientifiques et techniques de chaque pays. Ces documents, dont les extracteurs n'ont pas à apprécier la valeur, sont envoyés au secrétariat général qui fait un premier choix, et répartit la matière entre les mains de spécialistes qui sont chargés d'éliminer les documents sans intérêt réel et de mettre en tableaux les données classées par spécialités. Les tableaux du vol. IV ont ainsi été dressés par 27 savants, dont 20 français, 5 anglais et 2 suisses, sans compter la Commission permanente du Comité international et le Secrétaire général.

En vue d'assurer l'existence d'une pareille œuvre dont les dépenses sont extrêmement importantes, qui vécut jusqu'en 1914 de subventions d'académies, de sociétés savantes et de quelques gouvernements, et qui traîne en ce moment un passif assez gros, la constitution d'un fonds international fut proposée en juin 1922, et vient d'être définitivement approuvée dans la séance tenue le 27 juin 1923 à Cambridge par l'*Union Internationale de Chimie pure et appliquée*. Douze pays ont actuellement adhéré à ce fonds international (Belgique, Danemark, Espagne, Etats-Unis, France, Grande-Bretagne, Italie, Norvège, Pays-Bas, Portugal, Suisse, Tchécoslovaquie), et s'imposent de ce fait des contributions régulières, dont la plus lourde, et de beaucoup, est supportée par la France qui veut ainsi reconnaître l'honneur qui lui échoit d'être chargée de l'impression de ces *Tables*. (Le vol. IV fait vraiment honneur aux éditeurs GAUTHIER-VILLARS et C^ie^.) D'autres adhésions sont à peu près assurées à ce fonds international, notamment celle du Japon. Puissent-elles être suffisantes pour que continuent à vivre les *Tables annuelles* dont CH. MARIE est l'animateur.

L. G.

Reviews

Hippocrates. — With an English translation by W. H. S. Jones. (Loeb classical library) Vol. I, lxx, 362 p. London, Heinemann; New York, Putnam, 1923.

The editors of the Loeb classical library deserve warm congratulations for having included this edition of Hippocrates in their series. The publication of a handy volume containing good texts and translations of some of the most important or typical writings of the Hippocratic corpus would have been welcome in any case; it is doubly welcome when it forms a part of a collection as justly popular as the Loeb popular library. For example, I know American clubs which buy every volume of it and I like to think of business men reading some fragments of Hippocrates during their luncheon rest! This is not the first work of scientific interest included in that library. It contains also Galen's *On the natural faculties*, edited by A. J. Brock; Theophrastus' *Enquiry into plants*, by Sir Arthur Hort, Bart. (2 vol.) and Strabo's *Geography*, by Horace L. Jones (only 1 out of 8 volumes has yet appeared). I hope that the Association Guillaume Budé (1) will follow this good example. If it does, the teaching of the history of ancient science will soon become much easier. Indeed the instructors to whom such teaching is intrusted need just such handy volumes containing good texts and good translations, and easily available. They can hardly expect their students (not necessary classical students) to read long works in Greek or Latin, but they can ask them to read the greatest part in English or in French, and some of the most important fragments, if possible, in the original.

The editor of the Loeb Hippocrates is William Henry Samuel Jones, bursar and steward of S. Catherine's College, Cambridge, author of various Latin and Greek text books (2), but better known,

(1) See my review of the Lucretius edited in Latin and French by Alfred Ernout and published by that Association in 1920, *Isis*, IV, 47.

(2) Chiefly his *Via Nova* or the Application of the direct method to Latin and Greek. Cambridge University Press, 1915.

at least among historians of science, through his works on malaria in ancient times (3). He claimed in them that the decadence of Greece and Rome was probably caused by malarial diseases, a theory which he failed to prove completely but which was very suggestive and provoked a good deal of useful discussion. To come back to his HIPPOCRATES, he has endeavored « to make as clear and accurate a translation as the condition of the text permits, introducing as few novelties of his own as possible, and to add such comment as may bring out the permanent value of the various treatises ». He acknowledges the assistance of E. T. WITHINGTON (who has been engaged for many years in the study of Greek medical lexicography for the new edition of LIDDELL and SCOTT) who will probably edit himself one of the subsequent volumes.

The first volume is intented to be typical of the whole Hippocratic corpus. It includes, indeed, works of various kind, to wit : Ancient medicine (περὶ ἀρχαίης ἰητρικῆς); Airs, waters, places (πέρὶ ἀέρων ὑδάτων τόπων); Epidemics I and III (ἐπιδημίαι); the Oath (ὅρκος); Precepts (ἀφορισμοί); Nutriment (περὶ τροφῆς). The reading of these six works would give one already a good idea of the miscellaneous contents of the corpus. The editor has provided a good-sized introduction cantaining a summary of our present Hippocratic knowledge and each text is preceded by a special introduction wherein its characteristics are outlined and previous editions and translations are quoted. A splendid beginning! I look forward with some impatience to the following volumes.

G. S.

Erich Frank. — *Plato und die sogenannten Pythagoreer*. Ein Kapitel aus der Geschichte des griechischen Geistes. Halle (Saale). Verlag von MAX NIEMEYER, X und 308 S. 1923.

Ueber PLATO und seine Stellung zur Mathematik (im weitesten Sinne) ist von Philosophen, Philologen und Mathematikern schon so viel geschrieben worden, dass man glauben könnte, das Thema sei erschöpft. Das hier anzuzeigende Werk, das Erstlingswerk eines Heidelberger Gelehrten, beweist, wie viel noch an neuen Erkennt-

(3) *Malaria, a neglected factor in the history of Greece and Rome*. Introduction by Major RONALD ROSS; concluding chapter by G.G. ELLETT. VI + 107 p. Cambridge, MACMILLAN, 1907. *Malaria and Greek history*, to which is added the history of Greek therapeutics and malaria theory by E. T. WITHINGTON, VIII + 175 p. Manchester University Press, 1909.

nissen und Einblicken aus der kritischen Durchleuchtung der Texte zu gewinnen ist, wenn Philosoph, Philologe und Mathematiker in einer Person vereinigt sind.

Das Werk, aus zwei Teilen (S. 1-149), einem Anhang von zwanzig Beilagen (S. 150-334) und Anmerkungen (S. 335-385) bestehend, fordert vom Leser ernsteste Arbeit. Die Anordnung des Ganzen wird verständlich, wenn man die Beilagen als die grundlegenden philologisch-kritischen Untersuchungen wertet, die der zusammenfassenden Darstellung der Ergebnisse vorausgehen mussten. Aber ich wiederhole, es ist nicht nur die meisterhafte Beherrschung der philologisch-kritischen Methode, die man bewundern muss, auch der Mathematiker und Musikhistoriker haben an den vielfach neuen und überraschenden, fast immer auch überzeugenden Ergebnissen der kritischen Beleuchtung der Probleme ihren Anteil.

Der erste Teil behandelt die Entstehung und Entwicklung des rationalen Weltbewusstseins in der griechischen Kunst, Wissenschaft und Philosophie. Der Verfasser geht aus von der Entwicklung des musikalischen Bewusstseins. Mit Recht hebt er hervor, dass die herrschende Stellung, die die Musik im griechischen Geistesleben gehabt hat, nicht immer genügend gewürdigt wurde. Seine wundervolle Nachempfindung, Nachdichtung möchte man sagen, der musikalischen Stimmung des Griechentums verrät den begeisterten und kenntnisreichen Musiker. Freilich, der Sang ist verschollen, niemals wird unserm Bewusstsein jenes Getragensein des Griechen durch Musik und musikalische Tonfolgen so lebendig werden, wie Architektur und Plastik durch die Harmonie der Linien, der räumlichen und farbigen Erscheinung auf uns wirken. Wenn schon die Griechen selbst bald nach Plato das Verständnis für die Feinheiten der Viertelstonmusik verloren haben, wird es für uns trotz der allerneuesten musikalischen Expressionistik ein vergebliches Bemühen bleiben, jene verschollene Welt wieder wirksam nachzuformen. Aber wie tief überall Musik, Empfindungswelt und Ethos ineinander greifen, das lehrt uns ja jeder grosse Musiker, sei es Palestrina oder Bach, Beethoven oder Wagner, leider auch jeder Gassenhauer und jedes Massengeschrei.

In merkwürdigem Gegensatz zu der dämonischen, dem Verstand unfassbaren Macht der Töne steht die Tatsache, dass die Harmonie der Töne von ganz bestimmten Zahlenverhältnissen abhängig ist. Die praktische Kenntnis dieser Verhältnisse muss man schon in der älteren Musik voraussetzen, die richtige Erklärung darf man nicht dem alten Pythagoras, sondern erst den Pythagoreern, die Plato's Zeitgenossen waren, zuschreiben, vor allem dem führenden Geiste jenes Kreises, Archytas. Er hat in der Akustik zuerst die

Methode der modernen Physik, die Verbindung von mathematischer Deduktion und Experiment durchgeführt. Für PLATO werden die mathematischen Zusammenhänge zu Symbolen, oder mit GOETHES Worten zu reden, er eignet sich zwar das damalige höchste Wissen der Zeit an, aber er sublimiert es zur intelligiblen Welt der Ideen, « um die Forderung des ewig Ganzen, Guten, Wahren und Schönen in jedem Busen aufzuregen ». Er will nicht Wissenschaft, sondern Intuition, und nur soweit Mathematik den Geist von der Sinnlichkeit losreisst, ist sie ihm von unersetzlichem Wert.

Auf ARCHYTAS und seinen Schüler EUDOXOS wird auch die erste Formulierung eines Gesetzes der Planetenbewegung zurückgeführt. PLATO erzählt in den Gesetzen, dass er diese neue und eben so schöne wie wahre Entdeckung — dass die mit Unrecht « Irrsterne » genannten Himmelskörper vollkommene Kreisbahnen beschreiben — erst in hohem Alter kennen gelernt habe. Nicht tote Körper sind die Gestirne, sie sind beseelt; denn wie könnten sie ohne Seele so wunderbar genau ihre Bahnen beschreiben ? Kein Sterblicher kann zum Glauben an Gott kommen, wenn er nicht zwei Dinge vor allem erkannt hat : dass die *Seele* das Primäre aller Dinge ist, dass sie nicht stirbt und vor allen Körpern über alle Körper herrscht, und dann, dass ein Verstand in den Gestirnen ist, der sie nach mathematischen Gesetzen leitet, kein blinder Zufall, der sie von aussen stösst.

Wer immer diese Fragen berührt, wird auch auf jenen wundervollen Bericht im Vorwort von KEPLERS *Harmonice Mundi* zu sprechen kommen, jenes wahrhaften Pythagoreers und tiefsinnigen Sterndeuters, der vor kaum 300 Jahren der Welt seine Entdeckungen schenkte, und er wird von COPERNICUS zurückgeführt werden auf den kühnen Griechen, der als Erster die Erde um die Sonne kreisen liess. Vollendet wird das heliozentrische System durch ARISTARCH, als der griechische Geist schon alt und müde war. Nur wenig mehr als 100 Jahre nach der Entdeckung der Kugelgestalt der Erde war das Endziel erreicht, war das heute mit COPERNICUS' Namen erbundene Weltbild gefunden. Aber kein GIORDANO BRUNO, kein GALILEO oder KEPLER war damals bereit, die neuen Gedanken mit Leidenschaft und Tiefsinn weiter zu verfolgen. Der von dem Mathematiker SELEUKOS um 150 v. Chr. versuchte Beweis für ARISTARCHS Hypothese wird durch den Nachweis HIPPARCHS, dass keine Theorie die Planetenbewegungen vollkommen zu erklären imstande sei, erledigt und die geniale Konzeption zugunsten reiner Empirie verlassen. PTOLEMAIOS wird drei Jahrhunderte später mit seinem geozentrischen System zum Totengräber der einst so glänzenden Entwicklung.

Ich muss mir versagen, ausführlicher auf die Entwicklung des Begriffs vom Unendlichen einzugehen, die FRANK von ANAXAGORAS und DEMOKRIT und THEAITETS Entdeckung des Irrationalen bis zur in der modernen Philosophie und Physik führt. Unser wissenschaftliches Weltbild ist in der Zeit zwischen ANAXAGORAS und PLATO entstanden. Die immer wieder vorgebrachte Behauptung, die Griechen der klassischen Zeit hätten noch keine exakte Wissenschaft gehabt, und die allgemeine Spekulation habe sie damals ersetzt, ist falsch; die Sache verhält sich vielmehr umgekehrt: vor allem die mathematischen Wissenschaften nehmen im fünften Jahrhundert einen so schnellen Aufschwung, dass es den Philosophen unmöglich wird, dem Gang der Entwicklung zu folgen. Schon PLATO verzichtet bewusst auf jede produktive Mitarbeit an den Wissenschaften. Er verfolgt wohl die gewaltige wissenschaftliche Bewegung seiner Zeit mit leidenschaftlicher Teilnahme, in seinem Geist finden die umwälzenden Entdeckungen der Pythagoreer lebendigsten Widerhall, aber er vermag selbst kaum mehr mit dem Gang der Entwicklung Schritt zu halten. Die Vorstellung, als ob PLATO ein schöpferischer Mathematiker und Astronom gewesen sei, lässt sich nicht aufrecht erhalten. Er benützt wohl die Ergebnisse, aber sie sind ihm blosser Stoff, mit dem er in souveräner Willkür umspringt, den er ganz im Sinne seiner Ideen und Zahlenspekulationen umformt. Nichts verkehrteres als bei PLATO ein getreues Spiegelbild der zeitgenössischen Wissenschaft zu erwarten, oder der griechischen Wissenschaft auf grund von PLATOS Darstellung exakte Methode abzusprechen. (S. 65). Mit dem alten PYTHAGORAS aber hat diese ganze Wissenschaft nichts zu tun; die Behauptung einer rationalen Mathematik und Physik des alten Weisen ist eine literarische Fiktion wie die von der Philosophie HOMERS.

Ich muss mich, nachdem ich vom Geiste des Ganzen einige Proben gegeben habe, für den zweiten Teil, der die Philosophie der sogenannten Pythagoreer untersucht kürzer fassen. Es handelt sich vor allem um die Frage der Einwirkung der Pythagoreer auf PLATOS philosophische Entwicklung, insbesondere auf seine Naturphilosophie. Danach wird zunächst das Verhältnis von PLATO zu DEMOKRIT untersucht, und nach einer Darstellung der Naturphilosophie des ARCHYTAS das Verhältnis — zeitlich und sachlich — des PHILOLAOS und der andern sogenannten Pythagoreer zur platonischen Philosophie festgestellt. Die Darstellung schliesst ab mit einem Zitat aus LEIBNIZ und dem Nachweis, dass unsere ganze moderne Naturauffassung sich schliesslich auf den Pythagoreer ARCHYTAS zurückführen lässt.

Meine Inhaltsangabe, die dem Gedankenreichtum des Werks gegenüber dürftig genug ist, kann noch weniger dem Anhang gerecht werden, in dem die eigentlich kritische, grundlegende Arbeit des Verfassers in zahlreichen Untersuchungen niedergelegt ist. Vier Beilagen sind der Geschichte der griechischen Musik und ihrer Theorie gewidmet, neun der Geschichte der griechischen Astronomie, sieben der Philosophie und Mathematik der Pythagoreer. In diesem letzten Teil sind hervorzuheben die Untersuchungen über SPEUSIPPS System der Philosophie, über die Fragmente des HIPPASOS und besonders über die Fragmente des PHILOLAOS. Gegen BOECKH der bekanntlich vor Zeiten ihre Echtheit verteidigt hat, zeigt FRANK in Uebereinstimmung mit P. TANNERY, v. WILAMOWITZ, BURNET und andern ihre Herkunft aus der Mitte des 4. Jahrhunders. Sie müssen von einem Platoschüler verfasst sein; der einzige aber, der die Naturphilosophie der Akademie als Lehre des PHILOLAOS dargestellt hat, und dessen eigene Anschauungen nirgends mit den in den Fragmenten ausgesprochenen in Widerspruch stehen, ist SPEUSIPPOS, der Neffe und Nachfolger PLATOS, also muss er wohl auch der Verfasser sein.

Wie auch immer in Einzelheiten noch diese und jene Verschiebung eintreten mag, der Gesamtcharakter jener grossen schöpferischen Epoche der griechischen Wissenschaft ist durch FRANKS Meisterschaft in ihrem Verhältnis zur platonischen Philosophie klargestellt. Jede künftige Darstellung der Geschichte der Musik, Mathematik und Astronomie wird sich mit diesem Werke auseinanderzusetzen haben, wenn sie darauf ausgeht, nicht abstrakte Geschichte, sondern Leben und Wachsen des menschlichen Geistes darzustellen. Nur der Philologe, dem das Ganze der Volkskultur in ihren Höhen und Tiefen lebendig ist, kann die ältere Entwicklung und die gegenseitige Befruchtung von Philosophie und Wissenschaft so zu neuem Leben erwecken — allerdings auch nur *der* Philologe, der dem Verfasser gleich das volle Rüstzeug des Philosophen und des Mathematikers für seine schwierige Arbeit mitbringt.

(*Heidelberg.*) JULIUS RUSKA.

Emile Mâle — *L'art religieux du XII^e^ siècle en France.* Étude sur les origines de l'iconographie du moyen âge, IV + 460 p., in-4° carré, 253 gravures. Paris, COLIN, 1922.

[Broché : 50 fr.; relié : 85 fr.]

J'ai eu l'occasion, récemment (1), à propos du beau livre de M. LOUIS BRÉHIER, d'indiquer l'importance de l'étude de l'art chré-

(1) *Isis*, IV, 540-544, 1922

tien pour l'intelligence de la pensée médiévale. Le dernier ouvrage de M. Emile Mâle me permet de revenir à la charge. Les historiens du Moyen âge ne sont que trop portés, en effet, à se laisser absorber et envoûter par la scolastique. L'étude de la philosophie médiévale leur paraît le *nec plus ultra* et les meilleurs s'y fourvoient irréparablement; pensez donc à Duhem et à l'exemple plus récent que nous a donné M. Lynn Thorndike (1). Or, l'art du Moyen âge, si puissant, si plein de santé et de verve, me paraît le meilleur antidote de la scolastique. Autant celle-ci nous embrouille, nous déprime et nous fausse l'esprit, autant celui-là nous exalte, nous éclaire et nous instruit.

Il est intéressant de noter que ce livre, par lequel l'auteur aurait dû comencer son histoire de l'art religieux, soit le dernier à paraître. Son histoire de l'*Art religieux du* XIII^e^ *siècle en France*, qui le rendit célèbre, parut dès 1898 (5^e^ édition, 1923) (2). *L'Art religieux de la fin du Moyen âge en France* parut dix ans après (1908; 2^e^ édition, 1922). M. Mâle ne s'excuse pas de cette interversion, mais au contraire, il s'en félicite! C'est qu'en effet, s'il avait tenté d'écrire l'histoire de l'art français du XII^e^ siècle trente ans plus tôt, il se serait infailliblement égaré. « On croyait alors que l'art chrétien était né à Rome, et à peine commençait-on à entrevoir le rôle de l'Orient. Mais, peu à peu, la connaissance plus approfondie de l'art byzantin, l'étude des plus anciens manuscrits illustrés, l'analyse des ivoires des premiers siècles, l'exploration de l'Egypte chrétienne, la découverte des fresques de la Cappadoce firent comprendre que l'art chrétien ne devait rien au génie romain, mais qu'il était la double création du génie grec et de l'imagination syrienne... ». « Née en Orient, l'iconographie chrétienne nous est arrivée toute faite. Ce ne sont pas nos artistes qui, méditant sur le texte sacré, ont conçu les scènes de l'Evangile : ils les ont reçues d'un monde lointain. L'historien de l'art qui s'enfermerait dans la France du XII^e^ siècle se condamnerait à ne rien comprendre aux œuvres qu'il voudrait expliquer. Il doit sans cesse remonter aux origines, chercher en Egypte, en Syrie, en Cappadoce, les modèles dont il n'a souvent, dans nos églises, que la copie. »

Ceci nous introduit dans le vif du sujet et nous montre de suite la grande valeur historique des études iconographiques. Ce sont elles, en effet, qui nous fournissent le plus sûr des guides pour la détermination des voies par lesquelles la civilisation a été trans-

(1) Son livre est analysé plus bas.

(2) Traduction anglaise par Dora Nussey, Londres, Dent, 1913.

mise d'un peuple à l'autre. Et n'est-ce point là un problème tout à fait fondamental? Dans notre examen du passé nous voulons savoir avant tout comment les traditions qui sont la gloire et la joie de l'humanité se sont lentement établies, par quelles filières compliquées tous ces trésors sont parvenus jusqu'à nous. Or, le critique d'art nous donne, sur tout cela, des indications précieuses.

N'est-ce pas beaucoup, par exemple, d'avoir établi l'origine orientale de l'art chrétien? « Pour l'historien de l'art, ce qui marque la fin du monde antique, c'est le triomphe de l'Orient. On vit ressusciter l'art décoratif de la Chaldée, de l'Assyrie, de la Phénicie, de la Perse.... Dès les temps mérovingiens, c'est à l'Orient que l'Europe demanda des leçons. A l'art décoratif des Grecs, où tout était lumière, ordre, beauté, succéda un art étrange, où tout est rêve, demi-jour, mystère religieux.... » (p. 362).

M. MÂLE consacre son premier chapitre à la résurrection de la sculpture monumentale vers la fin du XI^e^ siècle, dans le Sud-Ouest de la France. Les abbayes clunisiennes de cette région furent très probablement son berceau. Moissac, par exemple, était un prieuré de Cluny et l'ensemble de sculptures qu'on y trouve fut vraisemblablement terminé avant 1125. C'est aussi par les prieurés clunisiens que fut propagé cet art nouveau. Dans la France du Nord, la sculpture monumentale apparut pour la première fois à Saint-Denis. Cette abbaye ne relevait pas de Cluny, il est vrai, mais nous savons que SUGER y fit venir des sculpteurs méridionaux. « L'amour de l'art est une des grandeurs de Cluny, qui en eut tant. » (p. III)... « Les Clunisiens avaient mille fois raison. Pour être pauvre comme Saint BERNARD, il fallait une merveilleuse richesse intérieure; l'humble fidèle avait besoin que l'on vînt à son secours. Qui pourrait dire combien d'âmes ont été émues, soutenues, consolées, au cours des siècles, par ces bas-reliefs, ces chapiteaux, où il y a tant de foi et tant d'espérance? Je l'ai senti vivement moi-même en déchiffrant ces vieux récits sur la pierre, dans la lumière des cloîtres ou dans le demi-jour des églises romanes. »

Les données iconographiques et les motifs décoratifs furent transmis aux sculpteurs français en tout premier lieu par les miniatures et subsidiairement par les étoffes persanes, byzantines, arabes, par les ivoires et les plaques de terre cuite. Les miniatures utilisées par eux étaient d'époques et de provenances très diverses, et ceci explique la complexité de leur iconographie. Il est assez facile, cependant, d'y reconnaître deux sources fondamentales: une source hellénistique — « Avant même la fondation de Constantinople, l'axe du monde s'était déplacé : l'Orient était redevenu la partie féconde

du monde antique. Dans les villes de l'Asie Mineure et de l'Egypte, où tant de races se mêlaient, le génie grec restait vivant. Ce prodigieux génie, qui semblait avoir tout créé et tout épuisé, inventait encore. Il paraît de plus en plus évident que le premier art chrétien est son œuvre » (p. 48) — et une source palestinienne, syriaque (1), — plus naïve, mais plus authentique et plus grande. C'est de Palestine, par exemple, que nous est venu le type de la Vierge souveraine, celle dont la maternité divine avait été proclamée au concile d'Ephèse, en 431, — la *Théotokos*. Au VI^e^ siècle, les deux sources commencent à se mêler, mais l'influence de l'art de Jérusalem ne cesse de grandir. L'influence byzantine ne se fait sentir que beaucoup plus tard. L'art de Constantinople se développe surtout du IX^e^ au XI^e^ siècle. « Par son fond, cet art est presque tout oriental, c'est-à-dire palestinien, syrien, cappadocien, mais par sa forme il est grec. Il ennoblit le réalisme de l'art oriental, et parfois l'on croirait voir revivre l'esprit de la Grèce » (p. 93). C'est au XI^e^ et au XII^e^ siècle que l'art byzantin a eu la plus grande puissance de rayonnement : il atteignit alors la Russie, l'Italie, la Sicile.

Après avoir ainsi étudié les sources de l'iconographie chrétienne, l'auteur consacre les six chapitres suivants (III à VIII) à l'étude de son enrichissement progressif : par les artistes français qui mêlèrent aux vieilles traditions leur jeunesse et les adaptèrent à leur génie; par la liturgie et le drame liturgique; par l'influence de SUGER; par l'hagiographie provinciale; enfin par les pèlerinages qui transportèrent les types nouveaux sur les routes de France, d'Italie et d'Espagne, et les modifièrent à proportion. Je ne puis m'arrêter à tout cela, mais il me faut dire quelques mots de SUGER, qui fut vraiment l'une des grandes personnalités du XII^e^ siècle. C'est grâce à lui que Saint-Denis a été, à partir de 1145, le foyer de civilisation que l'on sait. Il comprenait admirablement la valeur éducatrice de l'art et nous lui devons ce noble vers:

Mens hebes ad verum per materialia surgit.

Il avait une énorme érudition, mais son âme était assez forte pour en supporter le poids sans en être comprimée. Il était à la fois plein de raison et de passion; il est donc assez naturel qu'il aimât l'art. Il avait le génie de l'ordre et son biographe nous dit qu'il « aurait pu gouverner le monde ».

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(1) Voir à ce sujet *Isis*, IV, 543. La découverte du Saint Sépulcre et de la vraie croix, à Jérusalem, en 326, fut un événement d'une très grande importance. On pourrait le considérer comme l'origine de la suprématie des traditions iconographiques de la Syrie.

Le chapitre IX (pp. 315-363) nous intéresse tout particulièrement et nous l'aurions aimé plus étendu. M. MÂLE y considère les « tendances encyclopédiques de l'art », et nous y donne un résumé de la science de l'Ecole en tant qu'elle influença les artistes et, à travers eux, le peuple entier. Il nous y parle, par exemple, du microcosme et du macrocosme, de l'harmonie des sphères, du symbolisme des bestiaires, etc. Il montre que les animaux des chapiteaux et des portails n'ont cependant aucune signification symbolique, mais sont simplement empruntés aux tissus orientaux. Ces animaux n'introduisirent donc pas la nature dans nos églises romanes, comme on le dit souvent, mais plutôt la Chaldée et l'Assyrie, la Perse des Achéménides et des Sassanides, l'Orient grec et l'Orient arabe!

L'iconographie fut aussi fortement imprégnée par les idées monastiques (la peur de la femme, par exemple). Ce sont ces idées qui donnent à l'art du XII^e^ siècle l'apparence d'un art de visionnaires. En effet, le surnaturel tenait une place énorme dans la vie du moine. Il suffit de lire les livres de RAOUL GLABER, d'ORDERIC VITAL, de PIERRE LE VÉNÉRABLE ou la *Vie* de GUIBERT DE NOGENT pour s'en convaincre. Ceci fournit à l'auteur la matière de son chapitre X. Un dernier chapitre (XI) consacré aux portails historiés du XII^e^ siècle, nous donne alors une sorte de synthèse et de conclusion. C'est qu'en effet les grands tympans sculptés des portails romans étaient de vraies synthèses. Leur magnifique ordonnance préparait les fidèles à concevoir des pensées plus hautes que les soucis quotidiens. Celui de l'église de Mozat, près de Riom (Puy-de-Dôme), porte l'inscription : « Ingrediens templum refer ad sublimia vultum ». Les fidèles ne comprenaient point l'inscription, mais les grands ensembles sculpturaux parlaient tout droit à leur cœur. Or, ces ensembles furent presque tous conçus au XII^e^ siècle. Ce fut la gloire du siècle suivant de les porter à leur perfection.

L'ouvrage de M. MÂLE est admirablement écrit et illustré. Il complète dignement la grande œuvre à laquelle il a consacré sa vie.

GEORGE SARTON.

Theophrast von Hohenheim gen. PARACELSUS. — *Saemtliche Werke.* Herausgegeben von KARL SUDHOFF und WILHELM MATTHIESSEN. 1. Abteilung. Die medizinischen, naturwissenschaftlichen und naturphilosophischen Schriften, herausgegeben von KARL SUDHOFF. Sechster Band. Aus dem Jahre 1528 (Kolmar im Elsass). 198 S., 6 Taf. München. O. W. BARTH, 1922.

This is the first volume to appear of the new edition of PARACELSUS's complete works, which had been announced for many

years. As a matter of fact, the preface to this volume was written by its editor in January 1919. The edition of the scientific and medical works could not have been placed in safer hands than in those of KARL SUDHOFF who is, — one distinction among so many others — the leading PARACELSUS scholar of our time. His first *Paracelsus-Forschungen* (in collaboration with Eduard Schubert) appeared as early as 1889. They were followed in 1894 by a *Bibliographia Paracelsica* (printed works from 1527 to 1893) and in 1899 by the *Paracelsus-Handschriften.*

The present volume contains three works written by PARACELSUS at Colmar, Alsace, in the year 1528, to wit : (1) Drei Bücher der Wundarznei, Bertheonei; (2) Von allen offnen Schäden, so aus der Natur geboren werden, sieben Bücher (*de ulceribus*); (3) Von Blattern, Lähme, Beule, Löchern und Zittrachten der Franzosen und ihres gleichen, inhaltend zehn schöne Bücher (Von Chemie und Heilung der Franzosen neuen Bücher). The critical edition of these three texts is completed by the edition of the manuscript documents (letters, drafts) relative to them, by facsimiles of title pages and Mss. and by a historical introduction (p. 9-30).

Dr. SUDHOFF announces that the first five volumes of the scientific section will contain the writings of PARACELSUS's youth and of his Basel professorate. Four or five other volumes will be devoted to the works of the last four years and to those of uncertain date. A final volume will perhaps be added to the series, containing apocryphal writings. If all goes well (but does and can anything go well in Germany to-day ?) the whole of the scientific works should appear within five years. I look forward to the completion of this important undertaking.

The philological discussion of the text will be carried out by SUDHOFF'S coeditor, WILHELM MATTHIESSEN (Dr. phil. zu München) in the first volume of the theological writings. Dr. SUDHOFF will contribute a study on the authenticity of the scientific writings in the first volume of his own section. The publication of that volume will thus be expected with particular impatience.

GEORGE SARTON.

Hélène Metzger. — *Les doctrines chimiques en France du début du* XVII^e^ *à la fin du* XVIII^e^ *siècle*. Tome I, 496 p. Paris, Presses Universitaires de France 1923. [25 fr.]

M^me^ METZGER n'est plus une inconnue pour les lecteurs d'*Isis*. Ceux-ci se rappellent sans doute son beau livre sur les débuts de la cristallographie (1918) dont L. GUINET leur a rendu compte (*Isis*, III, 445); ils se rappellent mieux encore son mémoire sur l'Evolu-

tion du règne métallique d'après les alchimistes du XVII^e^ siècle (*Isis*, IV, 466-482; V, 231). Ce mémoire est une partie intégrante de son nouveau livre; il suffira donc de s'y reporter pour se rendre compte de sa manière.

Le sujet de cet ouvrage en est fort bien indiqué par le titre, mais le premier volume est consacré à peu près exclusivement au XVII^e^ siècle. Sans doute l'auteur n'a pu s'empêcher de parler de quelques savants du XVI^e^ siècle comme PARACELSE, CARDAN, BIRINGUCCIO ou le Gascon JOSEPH DUCHESNE (QUERCETANUS) et de quelques autres qui appartiennent plutôt au XVIII^e^, comme MALEBRANCHE, GEOFFROY, FONTENELLE, JOH. THOMAS HENSING (pas Hensig !) BOERHAAVE, PRIVAT DE MOLIÈRES et le P. BÉRAUD (1) mais cela n'est que tout naturel. En particulier elle a fait un excellent usage des vues rétrospectives de BOERHAAVE et de MALEBRANCHE. Il n'y avait pas de meilleur moyen de nous introduire dans le vif du sujet que de nous donner les conclusions de ces deux grands hommes dont l'intelligence égalait l'érudition et qui tous deux, surtout le premier, avaient vraiment vécu les querelles chimiques du XVII^e^ siècle.

Le but de M^me^ METZGER n'est point de nous expliquer les découvertes chimiques qui furent faites durant cette période et de nous la faire traverser ainsi en quelque sorte par une route royale. Au contraire, elle nous mène délibérément par le chemin le plus long. Elle n'est point comme ces voyageurs pressés qui ne s'arrêtent qu'aux capitales et aux endroits célèbres; elle préfère s'attarder dans la campagne, dans les bourgades les plus humbles. N'est-ce point là le seul moyen de comprendre un pays ? De même on ne comprendra bien l'histoire de la chimie que lorsqu'on tiendra compte non seulement des victoires décisives et des conquêtes retentissantes, mais aussi des mouvements confus qui se passent à l'arrière-plan. Elle nous raconte les malentendus et les querelles qui partageaient le public savant (ou soi-disant tel) du grand siècle, elle reconstitue « l'opinion moyenne des professionnels et des amateurs de la chimie sur chaque partie de leur doctrine »; en un mot, elle nous fait pénétrer l'atmosphère intellectuelle où s'élaboraient lentement les doctrines et que traversaient alors parfois comme des éclairs les découvertes réelles.

Une telle entreprise est aussi nécessaire que celle qui se borne à l'examen des progrès positifs. Toutes deux se complètent et l'histoire de la science à une époque donnée ne peut être considérée

(1) J'ai cité ces noms dans un ordre approximativement chronologique. L'œuvre du P. BÉRAUD nous amène déjà au milieu du XVIII^e^ siècle.

comme suffisamment connue que lorsqu'elle a été étudiée de ces deux points de vue. Il est donc ridicule de se chamailler là-dessus (1).

Le sujet choisi par l'auteur se prêtait d'ailleurs admirablement à sa méthode d'analyse, car il est difficile d'imaginer rien de plus confus que la philosophie chimique au XVII^e^ siècle. On pourrait dire sans aucune exagération que pour ce qui concerne la chimie, le moyen âge ne se termina qu'en 1675, et même après cette date l'énergie d'une ou deux générations de savants fut en grande partie dissipée dans des combats d'arrière-garde avec les derniers scolastiques (2). Les querelles chimiques du XVIII^e^ siècle nous font souvent songer aux querelles astronomiques du moyen âge, telles que DUHEM les a décrites, mais celles-ci sont relativement plus simples, moins médiévales. Ainsi la fameuse controverse entre les partisans des sphères homocentriques et ceux des épicycles est, en somme, fort raisonnable. On conçoit que les plus grands esprits du XIII^e^ siècle hésitèrent à choisir entre AL-BITRÛJÎ et PTOLÉMÉE; ROGER BACON ne put jamais se décider.

Quel était donc l'état d'esprit chimique au XVII^e^ siècle? D'après BOERHAAVE, qui parlait de longue expérience, il résultait de la fusion de deux courants distincts, l'un métallurgique (alchimique), l'autre médical. Chacun de ces courants était d'ailleurs fort complexe : on y trouvait une extrême droite mystique, Rosicrucienne, entièrement dépourvue d'esprit critique; une extrême gauche, plus ou moins positive et empirique, et entre les deux toutes les dégradations intermédiaires. L'opinion moyenne était un syncrétisme plus ou moins conscient. Comme aucune théorie n'était assez concluante ni assez ferme pour l'emporter, chaque chimiste, chaque homme du monde avait la sienne. Il choisissait dans le tas les vues qui lui paraissaient les plus plausibles et leur donnait une forme plus ou moins dogmatique selon son tempérament. Cette anarchie n'était tempérée que par la croyance vague mais générale dans l'unité de la science. On ne commence à voir un peu clair qu'en 1675 quand

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(1) De sages remarques sur ces deux points de vue ont été faites par le regretté P. BOUTROUX au sujet du développement de la mécanique, dans *Isis*, IV, 276 sq.

(2) Que la chimie médiévale fut bien lente à s'éteindre, on en trouve la preuve, par exemple, dans les Mémoires de l'aventurier vénitien CASANOVA DE SEINGALT, (1725-98). Ces Mémoires s'étendent jusqu'à 1774. CASANOVA n'était pas seul à se considérer comme un savant; cette illusion était partagée par une partie de « l'élite » européenne. Il avait d'ailleurs un esprit assez positif et des connaissances hétéroclites étendues, mais il était fort ignorant.

paraissent les *Experiments* de ROBERT BOYLE (son *Sceptical Chymist* date de 1680) et le *Cours de Chimie* de NICOLAS LÉMERY. Ce dernier ouvrage eut un succès prodigieux. Un passage de FONTENELLE (reproduit par Mme METZGER à la page 28) nous en conserve un écho bien intéressant. La gloire de BOYLE fut moins tapageuse, mais elle est beaucoup plus solide. En tout cas, il faut associer leurs noms; tous deux furent des libérateurs.

Je n'ai pas encore dit que l'enquête de Mme METZGER était limitée à la France. C'est ce que le titre nous dit. Ce n'est d'ailleurs pas tout à fait exact, car elle a tenu compte non seulement des œuvres françaises, mais de beaucoup d'œuvres étrangères traduites en français et même de quelques-unes qui, sans être traduites, ont cependant été passionnément commentées ou discutées par des chimistes français. J'ai déjà exprimé plusieurs fois mon hostilité pour les misérables Procustes qui mutilent l'objet de nos études en voulant l'ajuster à des cadres politiques dont il est essentiellement indépendant. Ces efforts deviennent particulièrement déplaisants quand ils sont inspirés par des sentiments chauvins, par un esprit de haine ou de dénigrement des autres peuples. Je me hâte de dire qu'il n'y a aucune trace de chauvinisme dans l'œuvre de Mme METZGER. Elle a restreint son enquête aux ouvrages français, parce que le sujet entier était trop vaste; il fallait donc faire quelque sélection. « Nous avons cru », dit-elle (page 9), « qu'en choisissant plutôt les livres écrits en notre langue, nous éviterions des contresens, si faciles quand on aborde un sujet presque inconnu; il nous semble d'ailleurs que l'accord des savants était si unanime qu'un historien d'un autre pays serait parvenu à peu près à nos conclusions. » C'est très probable, mais ce n'est pas encore démontré. En tous cas la sélection de l'auteur est justifiée par le grand prestige de la France aux XVII^e^ et XVIII^e^ siècles; une étude basée sur les ouvrages espagnols ou allemands de la même époque serait infiniment moins concluante.

Ce premier volume est divisé en deux sections : la théorie chimique avant LÉMERY et à l'époque de LÉMERY. Cette division n'est pas tout à fait satisfaisante, mais nous pouvons la négliger et considérer plutôt celle du volume entier en sept chapitres, comme suit : 1. La théorie chimique telle qu'elle s'enseignait au XVII^e^ siècle. Ceci comporte l'examen d'une série de cours de chimie, notamment ceux de JEAN BÉGUIN (Lyon, 1612) (1), de WILLIAM DAVIDSON (1657 ou 1651), d'ESTIENNE DE CLAVE (1646), d'E. R. ARNAUD (1650), de

(1) Les dates données par moi entre parenthèses sont celles des premières éditions françaises de chaque ouvrage.

Nicolas Le Fèvre (1660), de Christophe Glaser (1663), de Pierre Thibaut (1667), de Malbec de Tressel (1671). — 2. L'évolution du règne métallique d'après les alchimistes du XVII^e siècle (voir *Isis*, IV, 466-482; V, 231)). — 3. Aperçu des principales théories émises par les iatrochimistes. — 4. Développement de la philosophie mécanique et son influence sur la théorie chimique. — 5. La théorie de Lémery. — 6. La chimie expérimentale. Je disais que la division du volume en deux parties n'était pas satisfaisante. On le voit dans ce chapitre, car certaines œuvres expérimentales dont il y est question — les plus importantes — sont antérieures à Lémery. Celle de Jean Rey date de 1630, celle de John Mayow de 1668-1669. — 7. Triomphe et éparpillement des philosophies corpusculaires et mécaniques dans l'élaboration de la théorie chimique.

Etant donnée la manière de restreindre son sujet adoptée par l'auteur, il est intéressant de se demander: combien d'œuvres étrangères se sont imposées à son attention et quelles sont-elles ? Pour répondre à ces questions le plus simple me paraît être d'énumérer tous les auteurs du XVII^e siècle (1) dont M^me^ Metzger s'est occupée. Je les nomme dans l'ordre chronologique de leurs œuvres principales (autant que possible) et ceux de la même décade sont groupés ensemble; les noms des étrangers sont soulignés :

Jean Béguin.

Jean-Baptiste van Helmont.

Gassendi; Jean Rey.

Wm. Davidson; Mersenne; Descartes; Albara Alonso Barba; Pascal.

Estienne de Clave; E. R. Arnaud; Franz de le Boë (Dubois, Sylvius).

Johann Rudolf Glauber; François-Mercure van Helmont; Nicolas Le Fèvre.

Robert Boyle; Christophle Glaser; Michael Ettmüller; Robert Hooke; Otto Tachenius; Leibniz; Pierre Thibaut; John Mayow; Joh. Joachim Becher.

Malbec de Tressel; François André; Ole Borch (Borrichius); Nicolas Lémery; Du Clos; Chérubin; Claude Perrault.

Guillaume Lamy; Daniel Duncan; Bertrand; Friedrich Hoffmann.

Pierre Sylvain Leroy (Régis); Georg Ernst Stahl; Willem Homberg; Nikolaas Hartsoeker.

(1) Ceux des XVI^e^ et XVIII^e^ ont été énumérés plus haut.

Ce petit tableau est assez amusant, car il donne des résultats tout à fait imprévus. Ils étonneront l'auteur, j'en suis sûr, autant que moi-même. Parmi les 39 auteurs du XVII^e^ siècle auxquels M^me^ METZGER a donné le plus d'attention, la moitié (1) sont des étrangers. Si à ces auteurs on ajoute ceux du XVI^e^ et du XVIII^e^ siècle mentionnés plus haut, on trouve de nouveau que la moitié (25 sur 50) sont étrangers! Il est vrai, beaucoup de ceux-ci ont vécu une partie de leur vie en France, ou y ont étudié. La vie était fort cosmopolite au XVII^e^ siècle.

Mais continuons notre petit examen statistique. Il est intéressant à plus d'un titre. Parmi ces 25 étrangers qui influencèrent ainsi la chimie française, neuf sont des Allemands (SYLVIUS, GLAUBER, ETTMUELLER, TACHENIUS, LEIBNIZ, BECHER, FR. HOFFMANN, STAHL, HENSING); trois sont des Hollandais (HOMBERG, né à Java, HARTSOEKER et BOERHAAVE) et on pourrait grouper avec eux deux Flamands (VAN HELMONT, père et fils); trois sont des Anglais (BOYLE, HOOKE et MAYOW) et on pourrait y joindre deux Ecossais (WM. DAVIDSON et DANIEL DUNCAN; ce dernier était né à Montauban, à vrai dire, et écrivait en français); enfin PARACELSE et GLASER sont des Suisses; CARDAN et BIRINGUCCIO des Italiens (2); BARBA un Espagnol et BORCH un Danois; en tout huit ou neuf nationalités sont représentées.

L'examen par décade est aussi assez curieux. Les deux plus abondantes sont les septième et huitième, c'est-à-dire, la période s'étendant de 1661 à 1680. Parmi les neufs auteurs qui, d'après M^me^ METZGER, représentent le mieux la pensée chimique française de 1661 à 1670, huit sont des étrangers! Pendant la décade suivante la situation est renversée : nous y trouvons six Français et un étranger. Il ne conviendrait pas de trop insister là-dessus, car ma division en décades est nécessairement un peu artificielle, mais je l'ai faite sans aucune idée préconçue. *La morale de tout cela, c'est que la pensée scientifique est essentiellement internationale.*

Mon seul reproche à l'auteur, c'est que les indications biographiques et bibliographiques qu'elle nous donne sont vraiment trop sommaires. Elle a le droit d'éviter les détails biographiques, mais elle ne nous donne même pas l'essentiel! Il faut au moins que nous sachions clairement de qui elle parle; cela l'obligerait à donner

(1) Plus exactement, 20.
(2) Mais du XVI^e^ siècle, remarquez-le.

quelques précisions (1) et d'autant plus que beaucoup d'auteurs étudiés par elle sont justement tombés dans l'oubli. J'ai perdu pas mal de temps à identifier quelques-uns d'entre eux; cela est pénible. Par exemple, elle parle aux pp. 223-226 d'un LAMY, sans plus. Il s'agit de GUILLAUME LAMY de Coutances, qui obtint son doctorat en médecine à Paris en 1672. « HERMITE » cité à la page 110, n'est pas le nom d'un auteur. Il s'agit des « Aphorismes Chymiques.... Mis en ordre par les soins et le travail de l'Hermite du Fauxbourg. Nouvellement traduit du Latin en Français, par M. S. D. R. — Paris 1692 ». L'auteur de ce livre est peut-être LIMOJON DE SAINT-DISDIER ou F. M. VAN HELMONT (2).

Comme exemple de négligence bibliographique, je citerai les références de l'auteur à la Bibliothèque des Philosophes chimiques (3). Une première édition de cette collection attribuée au sieur S..., docteur en médecine, parut en 1672-1673. Une nouvelle édition en 4 volumes, Paris 1740-1754, fut préparée par JEAN MAUGIN de Richebourg. C'est celle-ci que l'auteur a employée. Or, cette édition est « revue, corrigée et augmentée »; ne contient-elle pas des interpolations? Et pourquoi Mme METZGER appelle-t-elle l'auteur SALOMON ? Quel SALOMON ? (4).

Mais, laissons ces détails. Il est évident que l'auteur devrait préciser les noms des auteurs dont elle parle et leurs dates. Il faut que nous puissions les situer exactement dans le temps et l'espace. Mais cela même ne suffit point. Je ne puis admettre que « l'histoire des doctrines chimiques est indépendante de l'histoire de la vie des savants ». Cela n'est vrai d'aucune histoire, nous ne pouvons jamais la dissocier sans danger de la vie des hommes auxquels elle se rapporte. Cela ne veut pas dire qu'il faudrait noyer l'exposé des doctrines dans des biographies. Non. Mais nous désirons savoir qui étaient ces hommes qui pensaient ainsi. D'où venaient-ils? L'auteur ne nous dit même pas s'ils étaient Français ou étrangers! A quel milieu social, religieux, intellectuel, géographique, professionnel appartenaient-ils? Tout cela est fort important à connaître et nous ne pouvons peser leurs paroles, si nous ne connaissons rien d'eux-mêmes. Son but n'est-il point de nous faire connaître l'atmosphère

(1) Souvent elle ne nous donne même pas les prénoms ou leurs initiales. Elle cite des livres sans mentionner la date. Certains noms sont orthographiés de façons différentes en divers endroits, etc.

(2) JOHN FERGUSON, Bibliotheca Chemica. Vol. I, Glasgow 1906, 379.

(3) P. 109-111, 118, 134-6.

(4) FERGUSON, *Ibidem*, vol. II, 1906, 272, 320.

intellectuelle des doctrines scientifiques, mais comment peut-elle y réussir sans nous présenter les hommes qui y vivaient et qui la transformaient sans cesse? J'espère de tout cœur que Mme Metzger s'efforcera de mieux satisfaire cette curiosité tout à fait légitime dans la seconde partie de son ouvrage. Et elle-même y gagnera une compréhension à la fois plus profonde et plus humaine de son sujet.

Quoi qu'il en soit, son livre est une contribution importante à l'histoire de la chimie. Il nous permet de reconstituer la pensée chimique moyenne du XVIIe siècle et nous fait admirer davantage l'énergie morale, le bon sens robuste de quelques hommes — tels Rey, Boyle et Mayow — qui ne se laissèrent point détourner par les grands courants intellectuels de leur époque et poursuivirent leur tâche sans peur ni reproche. La médiocrité du milieu fait ressortir la grandeur des héros.

George Sarton.

Douglas W. Freshfield (with the collaboration of Henry F. Montagnier). — *The life of* Horace Bénédict de Saussure. XII + 479 p., illustr. London, Edward Arnold, 1920.

A biography of Horace Bénédict de Saussure — one of the most interesting personalities of the second half of the XVIIIth century — had been wanted for some considerable time. The task of writing one was suggested by Ruskin to the author as early as 1878, but he took his time thinking of it and maybe he would never have completed it but for the assistance of Mr. H. F. Montagnier (1) who took considerable pains to obtain new material. The author himself may not be known to the average historian of science, but he needs no introduction to mountain climbers for the best part of his very long life was devoted to explore not simply the Alps, but some parts of the Caucasus and the Himalayas as well.

The subject is tempting enough. In the center of the stage, the personality of a great Genevese patrician; an original scientist; one of the founders of geology, of physical geography, of meteorology; the initiator of scientific mountaineering; an educational reformer. Then, behind him, Geneva at a critical period of her history; a tiny

(1) « An American gentleman known to his colleagues of the Alpine Club both as an assiduous climber and an accomplished student of the literature of mountain travel » (p. v.).

little republic rent by chronic disputes (1) between patricians and plebeians — the High and the Low Town; in fact, an oligarchy controlled by clerics and a group of noble families; a society full of charm and dignity, yet prudish, self-centered, clannish to an absurd degree. Finally within that extraordinary little town (2) a very remarkable group of men : Charles Bonnet, H. B. de Saussure, Théodore Tronchin; and close by, Voltaire !. Dr. Tronchin and later the Saussure family attracted distinguished people from all over Europe. It is true, the most illustrious Swiss scientist of that time, Haller, was pontifying, among the Germans, in Bern, yet he influenced Saussure so much, that a biography of the latter is necessarily full of references to the former.

To come back to Mr. Freshfield's book, it is very well done indeed. The account is based upon a critical study of the best documents available; it is well ordered and pleasantly written. The first chapter is devoted to Saussure's forerunners with regard to Alpine exploration. The earliest interest in the Alps was due to some Alpine monasteries and Baths. In the course of the XVI^th^ century, fifty treatises dealing with twenty-one different resorts were published. The great initiator was Leonardo da Vinci. Two other men of the XVI^th^ century deserve special mention : Conrad Gesner, who wrote in 1541 a letter on *The Admiration of Mountains*, and his pupil and biographer Josias Simler, who published in 1574 a description of Valais and the Alps. Some parts of the Alps of course had been diligently explored by crystal hunters. It is interesting to note, apropos of this, that Simler still wondered whether the crystals found in the Alpine crags might not be the result of intense and perpetual frost (3). After the Renaissance there was a period of reaction against mountains and against nature in general. Religious controversies absorbed the best of the intellectual energy. It seemed for a while as if a new period of scholasticism and obscurantism would begin. Happily some of the seeds sown in the XV^th^ and XVI^th^ centuries were too good not to come up, but their growth was very slow. Thus for the whole

(1) Happily these disputes were largely restricted to invectives. More ink was shed than blood. No fewer than 5885 political pamphlets appeared between 1735 and 1795! — In 1763, the *Contrat Social* and *Emile* had been officially condemned and their author banished.

(2) Population in 1781 : about 25,000.

(3) P. 13 This confusion may have been partly caused by the fact that the same Greek word κρύσταλλος meant at once rock crystal, ice, glacier (Strabo, XI, 6), — for men are prisoners of the words which they use.

of the XVII^th century, Mr. FRESHFIELD finds but one work worth quoting : the one known from its illustrator as MERIAN's *Topographia Helvetiae, Rhaetiae et Vallesiae* (1642) wherein the Grindelwald Glacier is, for the first time, carefully described and depicted. The main landmarks of Alpine literature in the XVIII^th century, before SAUSSURE's intervention, are : 1. JOHANN JAKOB SCHEUCHZER (1672-1733) *Beschreibung der Naturgeschichte des Schweitzerlands*, 1706-1708; Ὀυρεσιφοίτης *Helveticus sive Itinera Alpina tria;* 1708 (new ed., 4 vol., 1723); 2. GOTTLIEB SIGMUND GRÜNER (1717-1778) *Eisgebirge*. A popular edition of that work (entitled *Reisen durch die merkwürdigsten Gegenden Helvetiens*) appeared in 1778. SAUSSURE and later BERNARD STUDER spoke of it very highly; 3. HALLER's poem on the Alps, Bern, 1732, which obtained an immense success. One should also take into account the influence exerted by ROUSSEAU, though the latter never showed any interest in the High Alps.

H. B. DE SAUSSURE was born of a protestant Lorrainese family, at Conches, near Geneva, on February 17, 1740. His father was a well-to-do country gentleman, who had been a member of the Council of Two Hundred and had developed some scientific interest in agriculture (1). In 1760 the young HORACE visited Chamonix and, for the first time, had a nearer view of the « Accursed Mountains » (les Montagnes maudites). This sealed his fate. His life would be dedicated to the exploration and the scientific study of those mountains. Two influences were predominant in his career, that of his uncle BONNET and that of HALLER. In 1762, thanks to the latter's support, he was appointed professor of philosophy (2) at the Academy of Geneva. A letter written to his uncle five years later (aet.27), shows that the plan of his life's work is already outlined (p. 124) :

« Various tasks have retarted my Glaciers (*sic*). I am taking your advice, and writing down what I may forget and the points on which my impressions may grow faint. But I shall publish nothing for a long time. I realise that my work can only gain any value by the thoroughness of my investigations. Journeys have been made for more interesting objects, journeys more fatiguing,

(1) See CUVIER's notice in the *Biographie Universelle*.

(2) P. 75. From the Genevese point of view of that day, philosophy was held to include « psychology, logic, morals, and divinity, and also the general principles of the natural sciences with some acquaintance with the views of BACON, DESCARTES and LEIBNIZ ». A big order

more dangerous and more remarkable. What can I have to describe which has not been seen on a greater scale in the Cordilleras (1) and elsewhere ? What is wanted is a series of thoroughly carried out investigations into the causes of the low temperatures in the upper layers of the atmosphere, on electricity, on the chemical composition and the formation of mountains, on vapours, meteors, plants, animals. »

A good part of the book (p. 142-293) deals with SAUSSURE's Alpine travels. This is natural enough, considering the author's proclivity but even if one examines SAUSSURE's life from a broader than the purely Alpine point of view, the amount of space devoted to his mountaineering does not seem disproportionate. Even if these travels were only a means to an end, yet they were the very axis of his life. Chapter VIII wherein the conquest of the Mont Blanc is described, is particularly interesting. New documents have enabled the author to tell the true story. PIERRE BALMAT and MICHEL GABRIEL PACCARD, a village doctor, were the first to ascend the Mont Blanc, on August 8, 1786. The name of PACCARD should always be associated with that of BALMAT: theirs are equal fames. SAUSSURE himself made the ascent a year later on August 3. His object was scientific, rather than sportive. SAUSSURE did not do any mountaineering for its own sake, and while he was ever ready to endure discomfort, he eschewed the more dangerous feats. Yet the amount of climbing accomplished by him is considerable (p. 281) : « He probably covered more ground than any other Alpine traveller of his time, except, possibly PLACIDUS A SPESCHA » (2).

H. B. DE SAUSSURE, a born patrician, was a conservative, but far less so than HALLER; he was a sincere friend of orderly reform. « He was firmly convinced that the first necessity for the orderly development of a democratic state was the civilization through education of the demos » (p. 312). Hence his life-long interest in education. Early in 1774 he published his *Projet de réforme pour le Collège de Genève*, a liberal program which caused him to be regarded by his own party as a renegade and to be hailed with enthusiasm by the radicals. This program, it should be noted, was not accomplished until 1836. He was a true humanist in that

(1) A reference to the recent journeys (circa 1740) in the Andes of the French Academician, LA CONDAMINE, and his companions.

(2) A monk of Disentis. See *Pater* PLACIDUS A SPESCHA, *sein Leben und Schriften*. Bern 1913.

he realized fully the equal importance of scientific and classical studies. He saw clearly that both kinds of studies were necessary but neither sufficient. His artistic sense was hardly developed, it is true, but he was always quick to record traits of humanity. He was strongly opposed to the wave of materialism which was spreading from France over Western Europe, yet he was far more tolerant than HALLER. To complete this part of his portrait, it is worth while noting that he was the founder and the first president, in 1776, of the « Société des Arts » — which became a great educative center.

H. B. DE SAUSSURE, born rich and made richer still by his marriage, enjoyed all the advantages of birth and wealth in the most creditable way, but his last years were embittered by political misfortune and comparative poverty. The continual disputes caused by the aristocratic constitution of Geneva were brought to a head by the French revolution. The years 1789 to 1794 were years of deep unrest in Geneva. SAUSSURE lost his fortune invested in French securities. In 1794, he had a seizure. Geneva was annexed by the Directory in 1798 (1). His life, so prosperous and brilliant, was ending miserably indeed. Throughout these years of unrest he looked anxiously for scientific employment in various countries. Among the offers considered by him was one of JEFFERSON to come to his new University at Charlotteville, Virginia (p. 385). H. B. DE SAUSSURE died at Geneva on January 22, 1799. Triumphal funerals were accorded to him, yet, incredible as it may seem, both his family and his native city neglected to mark his grave, the exact spot of which is now unknown!

The main work left by H. B. DE SAUSSURE is his famous *Voyages dans les Alpes*, the publication of which span seventeen years of his life (2). It is a badly ordered, heterogeneous composition, yet one of fundamental importance. SAUSSURE is one of the founders of geology (3). He was the first (excepting, of course, LEONARDO) to insist upon the necessity of investigating the mountains where the successive strata have been twisted, broken and are exposed to our eyes : there was no better way, he showed, of

(1) It remained a part of France until 1813 when the Allies attached it to the Swiss Confederation.

(2) 4 vol. 4to Neuchâtel, 1779, 1786, 1796, 1796. Also printed in-8 vol., *ibidem*, 1780-96.

(3) By the way, he it is who introduced the terms geology, geologist in 1779. We owe to him also the term glacier, and the terms moraine and sérac (of Savoyard origin).

advancing the study of geology. PALLAS's treatise on *The Formation of Mountains* appeared, it is true, in 1777, two years before the first volume of the *Voyages*. SAUSSURE was able to take advantage of PALLAS's work, but he had reached independently similar conclusions and he had been the first to proclaim, if not to publish, them. On the other hand, he did not know of HUTTON's work; it is on the contrary HUTTON who took advantage of SAUSSURE's publications. While in Paris, in 1768, he met DESMARETS and GUETTARD, but his main master of « geognosy » was and remained WERNER. This explains how SAUSSURE continued until the end an inveterate Neptunian.

I touch here on a delicate subject. Mr. FRESHFIELD's book strikes me as being on the whole exceedingly fair, yet I wonder whether it is right to reproach SAUSSURE (as he does, e. g. on p. 151) for not having anticipated the Glacial Theory? It is astonishing indeed that the great Genevese never took practical steps to ascertain the nature of glacier movement or to measure its rate(1); one is amazed to discover that he believed that the middle of the glacial surface advances more slowly than its sides, when the contrary is almost obvious. One might reasonably have expected this great observer to have a better understanding of the very object of his studies. But is it not, on the contrary, unreasonable to expect him to discover the Glacial Theory — an immense generalization — which many scientist found difficult to accept when it was first explained in 1837 and 1840? If LEOPOLD VON BUCH and HUMBOLDT could hesitate to support AGASSIZ, is it just to blame SAUSSURE for having failed to anticipate him? (2) — No, the Glacial Theory could hardly have been discovered in the XVIII^th century. It was necessary to clear the ground first. Neptunianism and Catastrophism stood in the way. SAUSSURE was not able to understand glacial facts correctly because he had « water on the brain. » He, who had seen so many boulders moved by the advancing ice, never conceived the possibility of other boulders being moved in the same way! « He was too entirely convinced that all such phenomena owed their origin to gigantic catastrophes or floods, to

(1) And the more so because such experiments had been suggested by at least two contemporaries of him : (1) ANDRÉ CÉSAR BORDIER, a minister at Geneva in his *Voyage pittoresque aux glacières* (sic), 1773, which must have been in SAUSSURE's hands; (2) JEAN BENJAMIN DE LABORDE in his *Tableaux topographiques... de la Suisse*, Paris, 1780-88.

(2) ELIE DE BEAUMONT never believed in the Glacial Theory to the day of his death (1874)! See *The Life and Letters of* CHARLES DARWIN, vol. III, 250.

look for any less heroic agency (p. 430). » This is a new example of the limitation of genius, of how even the greatest men are the victims of their prepossessions.

The study of geology, and especially of mountain tectonics, led him to investigate atmospheric conditions. He thus became one of the founders of meteorology and the leading geologist of the Alps. He devised various new instruments (1) and had an extraordinary skill in manipulating them in almost impossible conditions. The series of observations made by him on the Col du Géant (11 030 feet) from the 5th to the 18th July 1788 was particularly remarkable; his camp there may be looked on as the earliest mountain meteorological station. I need not go into the details of these observations; nor is it necessary to speak of his fundamental work in the field of hygrometry (2). But it is worth mentioning, because little known, that he and his son believed that they found indications of a reduced proportion of oxygen at high altitudes, a belief confirmed by later investigations. He had shown also that water evaporated more quickly at high altitudes, a fact which is now applied in the vacuum pans for concentrating sugar solutions and the like.

In any final criticism of SAUSSURE's work it has to be born in mind that his activity was prematurely interrupted. He died at fifty-four and he had no time to complete his work. His *Agenda* published in 1798, is a pathetic document : the outlining of a task which he did not live to accomplish. To this, one may object that the essential of his work was probably done. He had conceived it before he was thirty and devoted more than twenty-five years to its execution.

Mr. FRESHFIELD, who can hardly forgive his hero for having failed to anticipate the Glacial Theory, sums up his opinion of him as follows (p. 277) : « In patience and accuracy of observation, in honesty and deliberateness in drawing conclusions, DE SAUSSURE was supreme. But in readiness to discard the theories of his predecessors, in quickness in drawing novel but legitimate inferences from the facts before his eyes, his intellect, it must be admitted, lacked the illuminating flash we term genius ». He lacked genius? But

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(1) Sling thermometer; hygrometer; slow action thermometer to measure temperature at the bottom of rivers or lakes, etc.

(2) *Essais sur l'hygrométrie*. 4to. XIV + 367 p., Neuchâtel, 1783. Also 8vo, XII + 524 p., Neuchâtel, 1783 — German translation by A. J. von OETTINGEN in OSTWALD's Klassiker, Nos. 115, 119.

is genius a matter of flash? Is it not rather the conception of a new task and the absolute dedication of one's life to it? The ability to conceive, the will to carry out. I prefer to think of genius as a continuous activity — moral as well as intellectual — than as an intermittent power of extraordinary vision. H. B. DE SAUSSURE did not make any startling revelations; his mind was inhibited by his absolute honesty, by his unwillingness to strive after effects, by his dread of premature guesses, but he showed new paths, he opened new branches of science, he was in many ways a true intellectual leader.

This excellent biography is completed by a note on SAUSSURE's meteorogical work and his observations on deep temperatures contributed by H. R. MILL; by a translation of the Preface and Epilogue and final Note of his *Voyages* — fragments of great biographical value — and by a list of his writings. It is very well illustrated.

GEORGE SARTON.

John Ambrose Fleming. — *Fifty years of electricity*. The memories of an electrical engineer. XII + 371 p., illustr. London, Wireless Press [no date; 1921]. [30 s.]

When one realizes to what a prodigious extent modern man is served — and dominated — by electric tools of every description, it is rather difficult not to forget the very newness of this electrification of our life. People who are not very old, our own parents, are living witnesses of preelectric days. Thanks to Mr. JOHN W. LIEB's courtesy it was my privilege, some years ago, to have an interview with Mr. T. A. EDISON in his library at Orange, New Jersey, and I have pleasant memories of many a Wednesday evening at the hospitable home of the late ALEXANDER GRAHAM BELL in Washington. I remember how hard it was for me to realize that these men, with whom I was talking, were two of the very originators of this age of ours, the Electrical Age. Yet our children take it already so much for granted, that for them preelectrical and medieval are almost synonymous!

The Electric Age may be said to begin about 1870. Prior to that year indeed, electrical engineering was practically limited to telegraphy, land and submarine (1). The dynamo had already been

(1) The only electro-technical society of that date was « The Society of Telegraph Engineers and Electricians ».

invented, but with the exception of a small amount of electric arc lighting (still in the experimental stage) and some employment in supplying electric current for electro-plating, there was no extensive use of it (1). There was no public distribution of electric currents, no electric lighting, no electrical transmission of power, no telephones and no electrometallurgy or electric heating. It is thus natural enough to choose this year 1870 as the starting point of a history of the electrical industry.

Few men were better qualified than the author of this book, to write it. This period of fifty years 1870-1920 (during which the material circumstances of human life underwent such a radical transformation), almost coincides with that of his own remarkable activity. JOHN AMBROSE FLEMING was born at Lancaster, England, in 1849. As a boy he was taken on an excursion down the Thames, to Millwall, to see the gigantic S.S. Great Eastern on the stocks, that is, the ship by whose means the first Atlantic cable was laid in 1866. I quote this little fact to show that he himself is a witness of the Telegraphical Age, I mean the age when electrical engineering was still limited to telegraphy. He thus watched the dawn of our own time. His first electrical paper dates of 1874 and he was closely connected with the introduction and development of three great inventions : the telephone, the electric incandescent lamp and wireless telegraphy.

He could have given an account of that momentous half century which would have been of permanent value. Unfortunately, he did not take sufficient trouble and his work is very disappointing. For one thing, one does not know which class of readers he had in mind when he wrote it. Sometimes he not only gives but repeats explanations of the most elementary nature (e. g. inductance is explained at least twice), then a few pages further his account becomes so technical as to cease to be readable. He is apparently a very poor theorist, but is at his best when he can describe an instrument or some technical detail. Yet even these descriptions are not always satisfactory. The book does not seem to contain much novelty, though one might have expected some good inside stories from a man who was so intimately associated with the events which he relates.

The book is divided as follows. An introduction of 49 pages tells the story of electricity during the previous half-century, that is from the days of WILLIAM STURGEON, OERSTED, JOSEPH HENRY, FA-

(1) The dynamo was perfected mainly during the years 1880 1890.

RADAY, etc., to 1870. This includes a brief history of the telegraph. The subject itself forms the substance of seven chapters : 1. Telegraphs and telephones; 2. Dynamos, alternators, transformers and motors, 3. Electric lighting; 4. Electric heating, cooking and furnaces; 5. Electric supply stations, storage batteries, railways and the transmission of power; 6. Electric theory and measurements (that is the worst part) 7. Wireless telegraphy and telephony.

The book is poorly composed and badly written. Historians of physics will hardly need it, though it may be useful, to a small extent, as a reference work. There is indeed a good index; the patents relative to the fundamental inventions are often quoted and there are many valuable illustrations, both photographs of machines and technical drawings. There are also a few portraits, including one of the author. In spite of the sub-title, autobiographical details are reduced to a minimum and this is another cause of disappointment. It saddens one to think that the author has actually lived that history and has so very little to say of it, beyond the technicalities which one may find in almost any textbook.

G. S.

Ananda K. Coomaraswamy. — *Catalogue of the Indian Collections in the Museum of Fine Arts, Boston*. Part I. General Introduction. VIII + 58 p. — Part II. Sculpture. 150 p., 86 pl. (bound in 1 vol., 30 × 22 cm.. Boston, Museum of Fine Arts, 1923. [$ 7.75.]

Dr. COOMARASWAMY, who was trained as a geologist and began his career in the geological survey of his native land, Ceylon, is now one of the foremost authorities on Hindu art, one of the best interpreters of the Hindu soul. Many of his very numerous publications have been briefly noticed in *Isis*, and two have been reviewed at greater length in vol. III, 403-407. Besides, the readers of *Isis* reap the fruits of his experience in another way, for he has kindly accepted to review himself briefly the main contributions to the study of the great Hindu civilization which he loves and knows so well. In his quality of « Keeper of Indian and Muhammadan art » in the Boston Museum, he is preparing an elaborate catalogue of the Hindu collections, of which these are the two first parts. The second is too technical to be reviewed in a journal which is not devoted to art. Let me say simply that it is prepared with considerable care and with a method, which reminds us of the author's scientific training. An introduction of 43 pages contains a history of Hindu sculpture; brief but sufficient outlines of the Hindu, Buddhist and Jaina mythologies; notes on

the use of images, iconography and Hindu sculptors; and a copious bibliography. Almost every object is illustrated. This catalogue will thus be truly indispensable to every collector or student of Hindu antiquities.

Part I, entitled « General Introduction », is of a more general interest. It is, in fact, within the brief compass of some fifty pages extravagantly spaced, a splendid summary of Hindu philosophy, religion and literature. It is not written for the Sanscrit or Buddhist scholar, but for the student of Hindu art and civilization. It is amazing to realize how much essential information the author has managed to compress in such small space, without any ugliness. This introduction is divided as follows : Vedic period, Upanishads Sannyasa and Yoga; Buddhism; Jainism; Brâhmanical theism (Hinduism); Tântrik systems; the Epics and Purânas; Drama; Aesthetic; Chronology; Bibliography.

The author combines in a very remarkable way the accuracy and the method of a scientist with the intuition of an artist. No wonder then that he is a master in the art of writing accounts, containing all the essentials and little more; terse and pointed but never dull. We look forward to the following parts of this catalogue, the whole of which will form a magnificent contribution to the study of Hindu art (1).

G. S.

Lynn Thorndike. — *A History of Magic and Experimental Science during the first thirteen centuries of our era*, 2 vol XLII + 835 p., VIII + 1036 p. New York, MACMILLAN, 1923. [Ten dollars.]

The author of this imposing work is Professor of History at the Western Reserve University in Cleveland, Ohio. He is well known to a large circle of students because of his *History of Medieval Europe* (from the decline of the Roman Empire to the opening of the XVI[th] century), an excellent textbook which appeared in Boston in 1917 (2). The publication of the *History of Magic* was not a

(1) The Catalogue is completed by a Portfolio of Indian Art containing 108 plates (104 in monochrome heliotype and 4 in color) of objects which Dr. COOMARASWAMY has selected from all the collection. This Portfolio is sold $ 35. at the Museum.

(2) Published by the HOUGHTON MIFFLIN CO., XXII + 682 p. THORNDIKE'S book entitled « Medieval Europe, its development and civilization », London, HARRAP, 1920, is simply a reprint of his « History of medieval Europe », with additional illustrations.

surprise for medieval scholars, but rather the fulfillment of a long-cherished hope. They knew that THORNDIKE was slowly elaborating it. His Columbia thesis of 1905 on « The place of Magic in the intellectual history of Europe » (111p.), was already an anouncement of it. A score of years had almost elapsed since, and their impatience had grown. The work is now before them, and it is, indeed, a very elaborate survey of magical ideas from the I^th^ to the XIII^th^ ceuntries, chiefly in the Latin world. Before discussing it, I wish to impress the reader with its importance. The best way to do this is to analyze it briefly. According to my customary method, I will mention the titles of every chapter, adding such information as may be needed to indicate its contents and minor criticisms which do not belong to the general discussion.

The work is divided into five books dealing respectively with the Roman Empire, Early Christian thought, the Early Middle Ages, the Twelfth century, the Thirteenth century. The substance of these five books is divided between 70 chapters numbered continuously, to which are added two additional ones serving as introduction and as conclusion to the whole work. The first four books are about of the same length (296 p., 211 p., 232 p., 300 p.) but the fifth is more than twice longer than any of them (664 p.). This is what one would expect, of course, and the more so in that THORNDIKE's investigations have carried him at some distance into the fourteenth century.

Chapter I is an introduction wherein we find a preliminary definition of the subject and a sketch of magical ideas in ancient times, in China, Egypt, Mesopotamia, Greece. The same chapter contains also some information (very imperfect) on early science. Our journey under THORNDIKE'S direction begins really with Chapter II, devoted to PLINY. The idea of taking the *Natural History* as the starting point of these studies on the decline and fall of ancient thought, seems to me excellent. PLINY stands, so to say, at the limit of two worlds. In spite of the fact that Greek science was not yet dead in his time and was soon to enjoy a Renaissance of many centuries, PLINY's work is already an anticipation of the medieval encyclopedias. It is at once a sepulchre of ancient science, and a cradle of medieval lore. SENECA and PTOLEMY must share Chapter III between them. They make strange bed-fellows indeed, but as there are no ways of curbing the fantasy of scholars, they themselves can not be held responsible for this. It should be noted that THORNDIKE'S account of PTOLEMY is largely centered upon the *Tetrabiblos*. He tries to justify this extraordinary course by saying that the *Geography* and the *Almagest* are too well known to require description, but that excuse is fallacious, for surely they are far less known than the *Natural History*. — Chapter IV, dealing with GALEN, is more satisfactory, but the author should have known that Books IX to XV of the *de anatomicis administrationibus* have been published in Arabic by MAX SIMON long ago (Leipzig 1906), together with an admirable Arabic-Greek-German vocabulary of technical terms. —

Chapter V. Ancient applied science and magic : VITRUVIUS, HERO, and the Greek alchemists. FRONTINUS to whom we owe one of the best fruits of experimental research in Roman days is not even quoted ! — It is *not* generally agreed that HERO belongs to the second century. And even the two authorities quoted by the author (HEATH and HEIBERG) agree now to place him in the third century and perhaps a little, if anything, earlier than PAPPUS (1). But THORNDIKE's work is so extensive that I must leave out other remarks of the same kind or we will never get to the end of it ! I must needs name the following chapters with as little commentary as possible : 6. PLUTARCH's essays (we are going back to the first century here ; I might notice once for all that the order of the narrative is chronological only in a broad manner, with various retrogressions) ; 7. APULEIUS ; 8. PHILOSTRATUS' life of APOLLONIUS of Tyana ; 9. Literary attacks upon superstition : CICERO, FAVORINUS, SEXTUS EMPIRICUS, and LUCIAN ; 10. Spurious mystic writings of HERMES, ORPHEUS, and ZOROASTER ; 11. Neo-Platonism and its relations to astrology and theurgy ; 12. AELIAN, SOLINUS and HORAPOLLO.

Book II. *Early Christian thought* : 13. Book of ENOCH ; 14. PHILO JUDAEUS ; 15. The Gnostics (SIMON MAGUS ; CARPOCRATES ; BASILIDES ; BARDESANES ; PRISCILLIAN) ; 16. The Christian apocrypha ; 17. The Recognitions of CLEMENT and SIMON MAGUS ; 18. The Confession of CYPRIAN and some similar stories ; 19. CELSUS and ORIGEN ; 20. Other Christian discussion of magic before AUGUSTINE (TERTULLIAN ; MINUCIUS FELIX ; LACTANTIUS ; HIPPOLYTUS ; JUSTIN MARTYR ; GREGORY OF NYSSA ; CHRYSOSTOM) ; 21. Christianity and natural science : BASIL, EPIPHANIUS, and the *Physiologus* (*Isis*, V, 278) ; 22. AUGUSTINE ; 23. Fusion of pagan and Christian thought in the IV. and V. centuries (J. FIRMICUS MATERNUS ; LIBANIUS ; SYNESIUS OF CYRENE ; MACROBIUS ; MARTIANUS CAPELLA ; DIONYSIUS THE AREOPAGITE).

Book III. *Early Middle Ages* : 24. The story of NECTANEBUS or the ALEXANDER legend in the early Middle Ages ; 25. Post-classical medicine (ORIBASIUS ; AËTIUS OF AMIDA ; ALEXANDER OF TRALLES ; MARCELLUS EMPIRICUS) ; 26. Pseudo-literature in natural science of the early Middle Ages (Medicine of PLINY ; *Herbarium* of APULEIUS ; Cosmography of AETHICUS ; *Geoponica ; De herbis femininis* ; *Macer Floridus*). The same chapter contains a discussion of the Dioscoridean tradition both Greek and Latin ; 27. Early medieval learning : BOETHIUS, ISIDORE, BEDE, GREGORY THE GREAT ; 28. Arabic occult science in the IX. century (AL-KINDĪ ; ABŪ MAʿSHAR ; QUSṬA IBN LŪQĀ ; THĀBIT IBN QURRA ; AL-RĀZĪ) ; 29. Latin astrology and divination, especially in the IX., X. and XI. centuries ; 30. GERBERT and the introduction of Arabic astrology (HERMANN's *De mensura astrolabii ;* ALCHANDRUS's *Mathematica*) ; 31. Anglo-saxon, Salernitan and other Latin medicine in MSS. from the IX. to the XII.

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(1) Sir THOMAS HEATH. Greek mathematics. 1921, vol. II, p. 306. (*Isis*, IV, 534.)

century; 32. CONSTANTINUS AFRICANUS; 33. Treatises on the arts before the introduction of Arabic alchemy (*Compositiones ad tingenda*; *Mappe clavicula*; HERACLIUS and THEOPHILUS); 34. MARBOD.

Book IV. *The twelfth century*; 35. The early scholastics: PETER ABELARD and HUGH OF ST. VICTOR; 36. ADELARD OF BATH; 37. WILLIAM OF CONCHES; 38. Some XII. century translators, chiefly of astrology from the Arabic (WALCHER; PEDRO ALFONSO; JOHN OF SEVILLE; GUNDISALVI; PLATO OF TIVOLI; ROBERT OF CHESTER; HERMANN THE DALMATIAN; HUGH OF SANTALLA; GERARD OF CREMONA); 39. BERNARD SILVESTER; 40. HILDEGARD; 41. JOHN OF SALISBURY; 42. DANIEL OF MORLEY and ROGER OF HEREFORD; 43. ALEXANDER NECKAM; 44. MAIMONIDES; 45. Hermetic books (KHÂLID IBN YAZÎD and the *Book of* MORIENUS; *Of the six principles of things*; *Liber lune*; *Book of Venus* of TOZ GRAECUS); 46. The Kiranides of KIRANUS, King of Persia; 47. Prester JOHN; 48. The Pseudo-ARISTOTLE (*Isis*, V, 214); 49. SOLOMON and the *Ars notoria*; 50. Dream-books.

Book V. The *thirteenth century*: 51. MICHAEL SCOT; 52. WILLIAM OF AUVERGNE; 53. THOMAS OF CANTIMPRÉ; 54. BARTHOLOMEW OF ENGLAND; 55. GROSSETESTE (with a note on WITELO); 56. VINCENT OF BEAUVAIS; 57. GILBERT OF ENGLAND and WILLIAM OF ENGLAND; 58. PETRUS HISPANUS; 59. ALBERTUS MAGNUS; 60. AQUINAS; 61. BACON (THORNDIKE minimizes the importance of BACON'S work and exalts ALBERT THE GREAT. I admit that some of his criticisms are just, yet I still believe that BACON was the greater man); 62. The *Speculum astronomiæ* (the author ascribes this treatise to ALBERT THE GREAT in accordance with medieval usage and concludes that it was written before 1277, — when Averroism was condemned by the Bishop of Paris, — and perhaps before 1256); 63. Apocryphal writings of ALBERTUS (*Experimenta* ALBERTI; *De mirabilibus mundi*; *De secretis mulierum*); 64,65. Apocryphal Experiments and Secrets of GALEN, RHAZES, etc.; 66. The *Picatrix*; 67. GUIDO BONATTI and BARTHOLOMEW OF PARMA; 68. ARNALD OF VILLANOVA; 69. RAYMOND LULL; 70. PETER OF ABANO; 71. CECCO D'ASCOLI (these two chapters 70 and 71 carry us already into the first quarter of the XIV. century; the one devoted to ABANO is one of the longest of the whole work, 74 pages); 72. Conclusion.

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The reader may have observed that I spoke of THORNDIKE'S investigations as if they were restricted to magic and did not include, as the title has it, experimental science. I have done so deliberately because the title is a deceiving one. This will be pretty obvious to students of the history of science, but alas! not to other scholars, and I must insist on it. When I had reached the second half of THORNDIKE'S first volume I was already convinced that this mention of experimental science in the title represented

an after-thought; I had detected a sufficient number of omissions and interpolations to make this quite clear to me. But it is not necessary to go into that, for the author admits it himself (on page 982 of vol. 2). « Originally magic alone was the object of my investigation, and experimental science an unexpected by-product which forced its importance during our period increasingly upon the attention. For this reason, while the magic of the learned has perhaps been treated here about as fully as it deserves, a complete and thorough history of experimental science through these thirteen centuries has not been attempted ». *Habemus confitentem reum.* It is of course better to make a clean breast of it late than never; but is it not altogether a strange procedure, to give a certain title to a book and then admit at the end of it (on the 1867th page! that is, at a point which not one reader in a thousand will ever reach) that the title is misleading? Such a confession should have been made in the foreword, or rather, the title should not have held out a promise which is not kept.

It is easy enough to prove that this promise is not kept. How should a history of experimentation be written? The bare mention of experiments without inquiry as to their real nature and as to the methods pursued is almost meaningless. It is as if a historian of art would deem his task complete after having told us that such or such an artist had produced an Annunciation, an Adoration of the Magi and a Theotocos. We would want to know *how* he treated these subjects; the bare mention of them could not satisfy us; in particular it could not give us any information on the progress accomplished by that artist. However strong this argument may be in the case of art, it is considerably stronger still in the case of science. *For science is essentially a cumulative, a progressive activity.* To bring out this essential feature of science, and above all of experimental science, it is necessary to explain and to discuss each step of the process. It is thus absurd to speak of a history of experimental science unless each experiment has been scrutinized as deeply as circumstances permit. A propos of ancient experiments we would ask ourselves, first of all, was the experiment simply suggested and outlined or was it actually made? Was it a real experiment? Was it well conceived? Was it complete, conclusive? How was it carried out? What was the degree of accuracy obtainable and actually obtained? etc. Has THORNDIKE tried to do anything of the kind? Nothing whatever. In fact, the futility of mentioning experiments without further ado, the necessity of explaining and discussing them, seem never to have entered his head. One might object that real experiments were very few

in medieval times, and that the available information concerning them is very limited. True (I will come back to medieval experiments presently); but this is no excuse. Granted that there was but little to say, THORNDIKE has failed to say even that little. In two or three cases he had a great opportunity but missed it altogether. The experimental study of refraction by PTOLEMY (or by the author of PTOLEMY's Optics) was one such opportunity; the experimental investigation of the spinal chord by GALEN was another. These are the most notable experiments of ancient times, and fortunately, a fair amount of precise information can be given about them; but THORNDIKE does hardly more than mention them (1). SYNESIUS' hydrometer is disposed of in the most casual way. A study of « experimental science » in the XIII^th century covering some 660 pages would naturally be expected to contain a full account and discussion of PEREGRINUS' experiments. The *Epistola de magnete* is mentioned *incidentally* (II, 791), but though THORNDIKE finds place to name MSS. of it which he has examined, he does not say a single word to suggest its importance !

These examples suffice to show that to call such a book a « history of experimental science » is a joke, and a very bad one at that. His brief allusions to astronomy are very unsatisfactory, and I did not find a single one to mechanics. Now, medieval mechanics was based on a few experiments, and nothing would have been more interesting than to explain the development of the various streams of thought relative to this, perhaps the most original development of medieval thought. I could not read THORNDIKE's book without realizing how fascinating a history of medieval science would really be, but we find hardly a trace of it in the book itself ! Indeed, a history of magic written by a physicist or an astronomer would have contained considerably more information on medieval science than this so-called history of experimental science !

Unfortunately, among the people who will read this book, by far the greatest number will be men whose scientific knowledge is about on the same level as the author's and who will thus be unable to realize how completely they are deceived. This imposing book will thus become an abundant source of misunderstanding, and will work considerable harm. This justifies the severity of my criticism.

To come back a moment to medieval experiments, in spite of the

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(1) Mind you, not less than 65 pages are devoted to GALEN !

fact that THORNDIKE explains more than once that the word *experimenta* (1) had not at all the same connotations in the Middle Ages as now, I can not help thinking that his gradual and unconscious falsification of his subject was perhaps partly caused by this and similar verbal ambiguities. The word *experimenta* has apparently some magical efficacy of which THORNDIKE was a victim among millions of others. Indeed, all men and, especially literary men, are likely to become the victims of the words which they use very often, and this fate becomes almost unavoidable when they have never taken the trouble of reaching a genuine and deep understanding of them. The student of semantics might ask himself why the meaning of that word developed as it did, and how it came to pass that medieval physicians speaking of *experimenta* meant simply remedies, recipes (experienced, tried remedies). Have you ever heard a quack (whether medical or political, it does not matter) who, extolling his panacea, did not repeat again and again that its efficacy had been fully tried? Even if they talk to old women, they will always insist that their discoveries are the results of experiments, and somehow, the less their hearers know what experiments really are, the more easily are they amenable to this magical word. THORNDIKE, investigating medieval ideas on magic, found the word *experimenta* so often repeated that he was imperceptibly beguiled to believe that he was studying experimental science as well and that the developments of magic and science were closely interrelated.

It is worth while to discuss this fallacy at some length, because it forms the very essence of THORNDIKE's book and because it is very important to have clear ideas on the relations of magic and science. We may accept the author's definition of magic (II, 974) and take it « to include all superstitious acts and occult sciences and to designate a great primary division or phase of human thought and activity ». In my opinion magic thus defined is fundamentally opposed to positive and experimental knowledge. To put it in the most compact form, the main characteristics of science are its rationality, its scepticism, its progressiveness, its limitedness; those of magic are its irrationality, its dogmatism, its unprogressiveness, its unlimitedness. The aim of science is the discovery, the unveiling of truth; as our power over nature is of

(1) Or cognate words, *experti*, *experimentarius*. It is interesting to observe that examples of various medieval meanings of the word *experimenta* are already found in PLINY (I, 54-57).

necessity a function of our knowledge of it, scientific research can but increase human efficiency, but the process is very slow. The aim of magic is power, immediate power, regardless of truth, of consistency, and even of morality.

The misunderstandings contained in THORNDIKE's book make me realize that the previous statements, though sufficient for the average man of science, need some elaboration. It is perhaps unnecessary to expatiate on the irrationality of magic, since this is generally admitted. In fact, the success of magic at all times and in all places is largely based on that characteristic, for men are irrational to a degree and even the most rational of them, think and behave irrationally whenever their nature ceases to be inhibited by logic (1). This explains at once that science is not a little repellent to the average man and that anything sensational, abnormal, shocking, impossible, fascinates him. Irrationality must necessarily appeal to beings who are normally irrational. Science is a joykiller. This prompts THORNDIKE to remark that utility is not magic's strongest appeal (II, 975) : « Much magic is done from want of anything better or else to do, rather than from complete conviction of its efficacy ». Very true. When I said that the aim of magic was power, I should have added, if it has any aim, — but most of it is aimless. Its irrationality includes aimlesness.

The scepticism and progressiveness of science are of course correlative, even as the dogmatism and the unprogressiveness of magic. At this point the confusion of THORNDIKE's ideas becomes apparent, for he speaks at one place (II, 983) of the indestructibility of thought, and at another (II, 973), of the evolution of medieval magic. Now, we can speak of indestructibility of thought only with regard to magic, because irrational ideas are in essence unimprovable. One irrational idea is as good as another. To speak of evolution of magic is thus a contradiction in terms. THORNDIKE concludes (II, 973) that magic became more and more scientific or pseudo-scientific in method and appearance, suggesting an evolution from the crudest magical ideas to the highest scientific ones. Now, I do not believe that he has been able to prove anything of the kind. The latest magical treatises are not by any means the most scientific. The fact that more importance is given by and by to astrology and less to magic proper would prove simply that magic was losing ground, not that it was itself more

(1) As, for example, when they fall in love or when their brain is paralyzed by any other passion or by the use of a drug.

scientific. Magic remained unalterably what it was: unreason. It might take new shapes, or revert to older ones, but the differences were but the most superficial; there might be change, but no evolution, no development. « Plus ça change, plus c'est la même chose ». On the other hand, the essence of scientific thought is its relativity, its transiency. Science is less a being than a becoming. Scientific truths are not meant to be absolutely true, but only more or less so. Each truth displaces and destroys an inferior one, and is in its turn destroyed by a superior one. Thus PTOLEMY's theory of refraction was *irrevocably* discarded as soon as the better theory of SNELL and DESCARTES was introduced. The changes here are not superficial but essential and not aimless but pointed toward a goal which does not vary much or moves but slowly. Each scientific truth which replaces another one brings one nearer to that goal. There is evolution, development, progress! The history of science is mainly the account of that progress; it is also the history of effective criticism with regard to accepted truths and to every form of unreason and credulity. That is, it may be conceived either as the growth of light, or else as the dissipation of darkness.

I said that science was limited, while magic was unlimited. This requires some explanation. If we believe in the unity of science, we can perceive a perfect science toward which our own is progressing. The total knowledge within our reach is in some ways limited, however extensive it be. Moreover, we know that the very progress of knowledge involves a gradual simplification of it. But at any time, to every truth, there may correspond an infinite number of errors. Human wisdom is limited; human folly, unlimited. That is one of the reasons why a history of magic is such a hopeless undertaking. Now, THORNDIKE seems unaware both of the specific unlimitedness of magic and of the limitedness of science. Thus, he says (II, 983): « Even the most intellectual men seem to have a limited number of ideas, just as humanity has a limited number of domesticated animals. Not only is man unable by taking thought to add one cubit to his stature, he usually equally fails to add one new idea to humanity's small collection. » There is here a double confusion. Unreason is unlimited, but it appears under a limited number of main forms; the combinations of these are almost innumerable, but can possibly be classified. Modern magic is but a new combination of the elements of ancient magic; not in any true sense a creation On the other hand, the ideal science towards which ours is developing, is limited, even as ours is, yet vastly superior in every respect to it. New elements are frequently

introduced; discoveries are made; new ideas are actually created. Take the history of any branch of knowledge, — optics, for example. The concepts of refraction, diffraction, dispersion, double refraction, light waves, aether, achromatism, aberration, interference, polarization, spectral analysis, quanta, etc., etc., were each of them, in their time, true creations, essential additions to our stock of knowledge. They were as new at the time of their discovery as America was in 1492. Each of them increased materially man's domain. It is because of that that the history of science is so fascinating; truly a history of victories and conquests; permanent conquests over the chaos; a growing light eating up the darkness. No other human activity lends itself to such a heroic and stupendous tale. Men of letters have no conception of this; in their little world, indeed, the number of ideas is limited, and every one of them has long been domesticated...

With the exception of magic, nothing is less progressive than education. Thus it happens that while science has made gigantic advances in modern times, educational methods are still largely in the medieval stage. Very few people, even among the professional scientists, have received a scientific education. No wonder, then, that the average man entertains the most confused ideas on the relations of magic and science! He has no means of understanding the difference between real and pseudo science, and in his eyes the faked goods often seem the most genuine. Everything is magic to him that he fails to understand. Yesterday he called VIRGIL, GERBERT, ROGER BACON magicians; to-day, EDISON is a wizard... Unfortunately, this confusion will not be corrected by THORNDIKE's book, but, on the contrary, considerably increased. The book itself is an admirable witness of it. Very few scientists will read it, even among those interested in the history of science, for the account of aimless aberrations of thought and of intellectual undertows repels them. THORNDIKE's readers will be mainly men unprepared to check his statements; their confused scientific ideas will thus become considerably more confused. After reading this elaborate parody of the history of medieval science, they will imagine that they know something of the subject, — yet they will know precious little of it, and their understanding of science will be worse than ever. Truly, such a prospect makes one shudder.

I must now complete this criticism of the subject as a whole by the examination of some specific misunderstandings.

THORNDIKE's main thesis is the co-existence, co-development, and interdependence of magic and experimental science. This thesis

is necessary to justify the subject of his book. If magic and science are independent, the development of each must be treated separately; it is absurd to mix both stories. The author is well aware of this, and reasserts his thesis more than once; yet he has signally failed to establish it. That magic and science co-exist is natural enough; they represent two essential phases of our intellectual needs. We find magical and scientific tendencies anywhere and at every time; thus they must needs co-exist. They may also influence each other to a very small degree by the very fact of their co-existence, often in the same brains, but I deny that this influence has materially affected their respective developments. Really, how could it? We have seen that science is essentially progressive, while magic is essentially conservative. There can be no compromise between them; they cannot possibly walk together — for one is going forwards, and the other backwards. All that they can do is to hinder and fight one another. One might thus properly write a history of the warfare of experimental science and magic, but it should be noted that THORNDIKE's thesis is exactly the opposite. He would make us believe that the magician (not he whom uneducated people called so, but the real magician) was the ancestor of the modern investigator; that reason is the fruit of unreason; and truth, of superstition and occultism. In fact, he has proved nothing beyond the co-existence of science and magic which we might have taken for granted : this co-existence is nothing indeed but the projection in the external world of the complexity and infirmity of our own mind. Which of the many magical ideas explained by him, did advance scientific research? On the contrary, nothing impressed me more in his book than the spectacle of ancient science gradually smothered by the luxuriant growth of medieval magic even as the flowers of our gardens, as soon as we cease to protect them, are slowly disinherited by the more tenacious weeds. Men are naturally irrational and lazy. Scientific thought is not natural to them, for it requires constant effort and inhibition. Even as our cultivated plants, it cannot be developed, unless its enemy — unreason, credulity — is fought out all the time. During the Middle Ages, the fight was a losing one, and the beautiful garden of ancient thought was gradually overrun with superstitions. Yet the fight went on heroically in many obscure corners of the world, and it would be fascinating to tell its vicissitudes, but THORNDIKE has done nothing of the kind. The weeds interest him too much; it is their story that he tells us; he would have us believe that they helped the flowers to grow! Nonsense!

Another source of error is THORNDIKE's continual confusion between magic proper on the one side and astrology and alchemy on the other. Now alchemy and even more astrology contained at least a nucleus of scientific thought. The fact that even this nucleus was proved to be erroneous by subsequent investigations does not matter much, for every truth is but relatively and temporarily true. Pure astrology was a very remarkable scientific system; it provided a congruous explanation of the world. The purer astrological and alchemical ideas date from Hellenistic times, and they received no material improvement during the Middle Ages. These theories could not develop because of their erroneous nature (sterility is the best mark of error); they remained substantially what they were; but as they blocked the stream of thought, they gathered around them all the superstitions which it carried. Thus more and more magic was added to them, like the detritus which congregate around the legs of a bridge. The scientific ideas forming the core of ancient astrology and alchemy influenced the progress of science to a considerable extent; they influenced it because of their soundness and congruity, and because they stimulated new observations and experiments. On the other hand, magic contributed nothing. A history of science which did not include an account of the purer aspects of astrology and alchemy would be essentially incomplete; but it is possible to write a consecutive and self-sufficient history of it without making even the slightest reference to magic. However, I hasten to add that the historian of science should occasionally refer to the state of religion or to the amount of superstition obtaining at each of the periods considered by him, in order to provide for his own story the proper background and relief (1).

The author seems to have derived some comfort for his thesis from the fact that magic is thriving to-day and that many people can still make a living out of it. Yet ours is a scientific age. Would this not prove that science and magic are interrelated? They are inseparable even as virtue and vice, morality and immorality. If

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(1) The three great domains — science, religion, and magic — may be briefly defined as follows : science is reason organized and systematically applied; magic is unreason; religion is a reasoned and not unreasonable abdication of reason with regard to questions which *are not* amenable to scientific treatment. There can be no conflict between science and religion, except when the latter trespasses upon the domain of the former, that is, when it involves an abdication of reason with regard to questions which *are* amenable to scientific treatment.

superstition still obtains in modern life, it is not because of scientific progress but in spite of it. It is clear that science can destroy prejudices and unreason only to the extent that it is applied, and persistently applied, to their destruction. The most efficacious remedy will not cure you if you do not take it. Scientific methods have been thus far hardly applied to the solution of social problems, and thus political fakirs are still the masters of our lives. Furthermore, the whole of education is almost untouched by science, and the greatest number of educators have no clear conception of it. Scientific and technical information may be obtained in almost every school, but I do not yet know a single place in the world where a true scientific education is dispensed. No wonder, then, that many men of science (let alone laymen!) think and behave in the most irrational manner as soon as they transgress the limits of their special studies (1). However, this does not prove that science is influenced by magic, but simply that it is undeveloped and that but few minds are truly and completely scientific.

This special misunderstanding is aggravated by the fact that every new scientific discovery invites some magical interpretations. This is especially true of discoveries which are of a revolutionary nature and appear at the first moment to shake the very foundations of science. Their strangeness, their esoterism, appeal strongly to all uneducated and unbalanced minds which seem to believe that the very accumulation of mysteries may cause some miraculous light to flash. Every new revolution of science revives their hopes. Thus, we have already a large literature dealing with radio-activity, disintegration and transmutation of elements, non-Euclidean geometry, and relativity from the magical point of view. Books have been written to infer the possibility of ghosts and of psychic phenomena from the « existence » of a fourth dimension of space (2). Not a few have been recently composed

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(1) This was abundantly illustrated during the war. One remembers the famous address « Es ist nicht wahr » signed by the leading scientists of Germany. Their colleagues of the allied countries who set themselves to prove that German science did not amount to anything did not show better judgment. In every case the critical faculty was inhibited by the exaltation of a noble feeling, patriotism.

(2) See also W. A. Granville's. *The fourth dimension and the Bible*, 128 p. Boston, Gorham Press, 1922 (reviewed in *American Mathematical Monthly*, vol. 30, 35-37, 1923).

by people who were themselves utterly unable to understand the theory of relativity, in order to explain its occult signification! (1).

Imagine a historian, writing a few centuries hence, who would examine carefully the magical literature of our days, and would conclude that it influenced the development of modern science. Would that not be extremely foolish? For we know well enough that these elucubrations have never influenced scientific thought even to the smallest degree. Now, that is exactly what THORNDIKE has been doing with regard to medieval science. From the coexistence of an abundant magical literature, of which he has made a deep study, and of some experimental investigations, of which his knowledge is very limited, he rashly concludes to a relation of cause and effect. It is infinitely more probable that in the Middle Ages, even as to-day, magical and experimental work were two activities fundamentally different, impermeable to one another. These activities were generally carried on by distinct persons, but it happened, then as now, that one personality reconciled the apparently irreconcilable and was in turn rational and irrational. This was nothing more, after all, than the exaggerated state of a normal duality. At any rate, even when we find magical fancies and scientific ideas mixed in the writings of a single individual, this alone does not justify our believing that there is some causal nexus between them. The existence of this nexus must be proved first. I do not remember having found in THORNDIKE'S work any such proof, or, in other words, any single case of magical activity having contributed anything to the progress of science.

To sum up this long discussion, I do not believe that THORNDIKE'S work « gives any new light on the origins of experimental science » (2). The ancestors of the scientists of to-day were not the conceited and foolish magicians, but the craftsmen trying honestly to solve concrete and definite problems.

Of course, the complexity of human nature is infinite, and some of these craftsmen may have been more or less addicted to magical practises; but it is as craftsmen, not as magicians, that they made discoveries. And even of those who posed as magicians or were considered as such by the crowd, how many were real magicians? May we not believe that the magic was only on one side as when a

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(1) Wise remarks have been made recently by PAUL PAINLEVÉ on the danger of studying new scientific theories without the necessary preparation. See *Isis*, V, 554.

(2) This claim is made on its jacket.

present-day « magician » extracts live rabbits from a silk hat? That is magic for the little boys who look, but not for the performer. Such suspicion is amply confirmed by the *Philosophumena* of Saint HIPPOLYTUS (written c. 222). Many medieval magicians and occultists were probably as foxy as the clairvoyants who pry upon our contemporaries. At any rate, this is a side issue. Whether honest or not, magicians did not influence science. THORNDIKE's asserts that they did, but gives no proof of it. Medieval science was certainly not free from magic, but it progressed, not because of this magical element, but in spite of it. THORNDIKE's investigations would have been more frutiful if his point of view had been primarily that of science instead of being primarily that of magic; that is, if he had explored how scientific ideas became gradually disentangled from magical fancies and from other forms of superstition and unreason. He would then have seen the evolution of knowledge in its true perspective, instead of seeing it, so to say, upside down. Instead of giving us the depressing recital of unprogressive and hopeless activities, he would have told us how mankind emancipated itself slowly from them. This would have been a tale of conquest, of hope, of joy.

If I must judge THORNDIKE's book on the basis of its title, it is undoubtedly, in spite of its learning, unsatisfactory, since the main point of a history of experimental science is entirely missed. It would not be unfair to judge it so, for it is the author, not I, who chose the title. However, it seems more generous to me not to take the author at his own word : instead of accepting his title and damning the book, I prefer to do the contrary. It would be very foolish, also, to throw away a real treasure of information, because it happens to wear the wrong label. I remember someone who insisted on calling champagne a brand of sparkling cider. I strongly resented this incorrect appellation, but the cider was very good; I forbore criticism and drank with great pleasure. I invite the readers of Isis to treat THORNDIKE's book in the same manner.

A few final words with regard to its form. It is in general carefully written, but I have the impression that the various chapters have not always received their proportional share of attention. Some seem very unequally developed, while others lack the finishing touch, and thus the beauty of the whole is spoiled. It is a pity that after twenty years of patient work, the author has denied himself the two or three additional years which would have sufficed to recast the valuable fragments into a harmonious whole. At any rate, every medieval scholar, every historian of science and

civilization, will be henceforth beholden to THORNDIKE for having examined with so much zeal and ingenuity an enormous amount of repellent material. His work contains a treasure of bibliographic information, abundant and precise. Many appendixes give lists of manuscripts and early editions. The author has himself examined a great number of these manuscripts, and a special index makes it very easy to refer to them. The general index seems excellent (1). I have tried it in many ways and have never been disappointed. Thus this work may be properly considered an encyclopedia of medieval lore, and as such it will prove very valuable indeed.

GEORGE SARTON.

Fester, Gustav. — *Die Entwicklung der chemischen Technik bis zu den Anfängen der Grossindustrie.* Ein technologisch-historischer Versuch. Berlin, 1923, JULIUS SPRINGER. VIII und 225 S.

Eine nach den besten Quellen für die Einzelgebiete und aufeinanderfolgenden Perioden mit selbständiger Durchdringung des Stoffs anziehend geschriebene Schilderung des allmählichen Aufschwungs der chemischen Technik bis zur neueren Zeit. Die Kennzeichnung der älteren Perioden, der chemischen Technik im Altertum und Frühmittelalter, stützt sich in erster Linie auf die Werke von LIPPMANN, KOPP, BERTHELOT, DIELS, NEUMANN, NEUBURGER, BLÜMNER, BUCHER und den alten BECKMANN; für die chemische Technik im späteren Mittelalter, bis zum Beginn des 17. Jahrhunderts, auch auf volkswirtschaftliche Werke wie HEYD, Levantehandel und SCHERER, Welthandel, auf ROSCHER, SCHMOLLER, CONRAD und SOMBART. Aus der mit dem 16. Jahrhundert beginnenden engen Verbindung zwischen Chemie und Medizin (sie bestand übrigens, nach theoretischen und praktischen Gesichtspunkten, auch bei den Muslimen), geht die präparative pharmazeutische Technik hervor, aus der sich schliesslich die moderne Präparatenindustrie entwickelte. Zugleich beginnt auch eine selbständige technologische Literatur, die sich — im Wesentlichen in Deutschland und Italien — ebenso eingehend mit dem Berg- und Hüttenwe-

(1) It would have been better, however, to combine the indexes of both volumes into one.

sen, wie mit der Destillierkunst und der Herstellung von Arzneipräparaten befasst.

Es liegt im Wesen einer Tausende von Einzelheiten behandelnden Schrift, dass man über sie schwer referieren kann, und dass jeder, der sich mit einzelnen Gebieten beschäftigt hat, Verbesserungen und Richtigstellungen nachtragen könnte. Von besonderm Interesse dürfte die Darstellung der Färbetechnik und der Gewinnung der organischen Farbstoffe sein. Mit dem 17. Jahrhundert beginnt die Verschiebung der wirtschaftlichen Suprematie nach Holland und England, der dreissigjährige Krieg verwüstet den Wohlstand Mitteleuropas und begründet Englands Weltstellung. Die Menge der durch die Fortschritte der Chemie und Technik in grösserem Massstabe gewonnenen Stoffe lässt hier noch weniger wie für die vorangehende Periode eine kurze Berichterstattung zu.

Heidelberg. JULIUS RUSKA.

Paul Helbronner. — *Description géométrique détaillée des Alpes françaises.* Tome I. La méridienne de Savoie, 1 vol. in-4° avec 5 pl. et 18 tours d'horizon photographiques dont 14 de 2m60 de longueur. Paris, GAUTHIER-VILLARS, 1920. Annexe du tome II. Les origines iconographiques de l'œuvre géodésique, XXIII feuilles (66 × 55 cm.) en 4 couleurs. *Ibidem*, 1921, Résumé des opérations exécutées jusqu'à la fin de 1911. (Thèse, Paris), 175 p., in-4°. *Ibidem* 1912. La description géométrique détaillée des Alpes françaises. De la 7e à la 15e campagne (1909-1920). *La Montagne*, janvier 1921, 45 p., 1 pl ; 16e campagne (1921) *Ibidem*, novembre 1921. 7 p., 1 pl. (Paris, Club Alpin Français).

Il importe de signaler ici l'œuvre magnifique à laquelle le polytechnicien PAUL HELBRONNER a consacré sa vie... « Laissant l'artillerie pour la peinture, amené par l'art à la beauté de la montagne, par la montagne à l'alpinisme, par l'alpinisme aux mathématiques et à une géodésie passionnée sur les plus hauts et difficiles sommets, élargissant ainsi la devise du montagnard : marcher, sentir, trianguler, peindre..., et écrire ». A l'exception de quelques années de guerre durant lesquelles l'auteur a rendu de grands services à l'organisation des services géodésiques au front, voilà plus de vingt ans qu'il se dévoue avec une énergie passionnée et inlassable à la géodésie, la photographie et la description des Alpes françaises. Au cours des quinze premières campagnes, M. HELBRONNER avait déjà relevé dans les hautes montagnes une surface d'environ 9,200 kilomètres carrés, occupé 1,286 stations géodésiques dont 559 au-dessus de 2,000 mètres et

131 au-dessus de 3,000, déterminé 6,058 points trigonométriques par au moins trois directions azimutales et recueilli plus de 10,000 clichés photographiques! De plus il fit tous les calculs pour la compensation analytique des polygones géodésiques, établissant ainsi — lui, simple particulier — le réseau primordial que la France elle-même n'avait pu établir, quoique la Savoie en ait fait partie depuis plus d'un demi-siècle. Un sous-produit tout à fait admirable de cette œuvre nous est donné dans l'Annexe du Tome second qui contient 24 planches en couleurs de très grand format, dont 13 représentent le tour complet d'horizon du sommet du Mont Blanc et les autres des vues du Mont Blanc de différents côtés, le tout basé sur les aquarelles, les photographies et les dessins exécutés par l'auteur, de 1899 à 1903. C'est une évocation merveilleuse de la Reine des Alpes. M. HELBRONNER est un savant et un artiste, c'est aussi un poète. En 1920, se trouvant au sommet de l'aiguille du Tour Noir, il se rappela les nobles paroles qu'elle inspira, en 1876, à E. JARELLE et son cœur y répondit : « Faisons notre minuscule partie dans l'immense concert, à la place où pour quelques instants éphémères, sans donner notre avis, nous avons été installés avec l'instrument que nous n'avions pas demandé; mais de cet instrument, jouons sans défaillance, le mieux et le plus longtemps possible pour la consolation suprême de notre conscience emprisonnée et inquiète: Travaillons! » L'instrument auquel fait ainsi allusion ce poète des Alpes n'est point la lyre, mais le théodolite! — Je suis très reconnaissant à mon ami M. EMM. DE MARGERIE de m'avoir fait connaître cette grande œuvre et ce grand caractère.

G. S.

Sir William Abbott Herdman. — *Founders of Oceanography and their work*. XII + 340 p., 29 pl. London, ARNOLD. 1923.

This very readable book is neither a treatise of oceanography nor a complete history of the subject. It is an « introduction to the science of the sea ». It is valuable for the historian because it is written by one who has devoted the best part of his life to these studies and has known personally some of the early protagonists. It is based upon a course of about twenty lectures given in the winter of 1919-1920 while the author held, for the first year, the newly established Chair of Oceanography in the University of Liverpool.

Of its seventeen chapters, the first six (or even seven) are historical, the others give introductory information on some of the most interesting aspects of oceanography : hydrography, ocean currents (with special reference to the Gulf Stream),

submarine deposits, coral reefs and islands, luminescence, plankton, aquiculture, sea-fisheries, food matters in the sea. The account is very clear and well illustrated.

Sir WILLIAM is not particularly interested in what we might call the prehistory of oceanography and disposes of it a little too briefly, very incompletely indeed in the first chapter. When did oceanography begin ? If we were to belive JOHN YOUNG BUCHANAN (1), it is scarcely half a century old, for it was born at sea on February 15, 1873, at the first official dredging station of the Challenger expedition!

That is an extreme view, which is about as unreasonable as if we claimed that a man does not exist until he has reached his maturity. Sir WILLIAM is less intolerant but he attaches but little importance to pre-nineteenth century work. It is worth while to reproduce here his table of the expeditions which contributed most to our knowledge of the sea during the century preceding the cruise of the Challenger:

Date.	Ship.	Captain	Naturalist
1768 71	« Endeavour »	COOK	Sir JOSEPH BANKS.
1831-6	« Beagle »	FITZROY	CHARLES DARWIN.
1839-42	« Porpoise »	WILKES	J.-D. DANA.
1839-43	« Erebus » « Terror »	JAMES C. ROSS	JOSEPH HOOKER.
1846-50	« Rattlesnake »	STANLEY	T.-H. HUXLEY.
1860	« Bulldog »	Mc CLINTOCK	G.-C. WALLICH.
1868	« Lightning »	MAY	WYVILLE THOMSON and W.-B CARPENTER.
1869-70	« Porcupine »	CALVER	WY. THOMSON, CARPENTER, GWYN JEFFREYS.
1872-76	« Challenger »	NARES	WY. THOMSON and others.

The history of oceanography in the XIX^th^ century is divided by the author in three periods : the period of EDWARD FORBES (1815-1854), the period of WYVILLE THOMSON (1830-1882), culminating in the Challenger expedition, and the post-Challenger period of Sir JOHN MURRAY (1841-1914). Sir WILLIAM's history is essentially, as you see, a British history, but as he does not claim to give a complete account and as the British contributions were undoubtedly the greatest, we will not quarrel with him on that score. Besides, if three chapters are chiefly devoted to the British contributions,

(1) The sole surviving member of the civilian scientific staff of the Challenger. See his Comptes Rendus, Cambridge 1917, and Accounts Rendered, Cambridge, 1919.

the three following ones deal respectively with LOUIS and ALEXANDER AGASSIZ, ALBERT OF MONACO and ANTON DOHRN. In a later chapter, devoted to sea-fisheries, he makes frequent allusions to Scandinavian and American investigations; he pays full homage also to the splendid results obtained by the French breeders of oysters and mussels.

EDWARD FORBES was born of Scottish stock at Douglas, in the Isle of Man in 1815. His father was a local banker. The boy showed very early a strong bent towards the study of nature and so much ambition that his grandmother TEARE, who had seen him grub for snails in a hedge, is reported to have said : « Ta mee credjal naugh vod slane Ellan Vannin sauail yn guilley shoh veich cheet dy ve ommydan (1) ». Grandmother's prediction proved to be untrue however, unless one be prepared to say (and many people undoubtedly are), that his efforts to become a great scientist were a sufficient proof of his foolishness. FORBES defined, in the Aegean, eight zones of depth characterized by peculiar faunas. Much of this is still accepted, except of course his « Azoic zone » — « he conjectured that the zero of animal life would probably be found somewhere about 300 fathoms ». His was a very brilliant and inspiring personality; his mind was sufficiently broad and imaginative and he had sufficient knowledge in every department of natural history to become the pioneer of a new encyclopaedic discipline.

Sir C. WYVILLE THOMSON, who was like FORBES, professor at the University of Edinburgh, is already much nearer to us. He was born in 1830, not far from the Firth of Forth, and died in 1882. He was educated at Merchiston Castle School (NAPIER's home). He was led to oceanography by his interest in Echinoderms, his study of fossil crinoids obliging him to investigate the living ones. His *Depths of the Seas* (1873) may be regarded as the first general textbook of oceanography. With the aid of his collaborators of the Ligthning (1868) and the Porcupine (1869-1870), he demonstrated that there is no azoic zone in the sea, but that numbers of animals are found living down to the greatest depths of five or six miles from the surface. He predicted, in 1880, the existence of the so-called « WYVILLE THOMSON Ridge », dividing the Faroe Channel in two areas, warm and cold. This was completely confirmed by the « Knight Errand » expedi-

(1) For the benefit of those readers who are not conversant with MANX, I append the following translation : « I believe the whole Isle of Man cannot save this boy from being a fool ».

tion (1880) conducted by captain TIZARD and JOHN MURRAY. The discovery of entirely different faunas living on either side of the Ridge, stimulated oceanographic research to a great extent. Sir WILLIAM describes briefly the Challenger expedition and his main results; I need not insist on this except to remark that the immense signification of that famous expedition, the greatest oceanographic expedition of all times, is becoming more and more obvious as years go by. It will remain one of the capital landmarks in the history of the XIX^th^ century. The life of Sir JOHN MURRAY, the third great protagonist, was largely devoted to the study of the Challenger collections. Thus two Scots share the honour of having directed the Challenger undertaking. JOHN MURRAY was born in Canada, at Coburg, Ontario, in 1841; he was killed in a motor accident in 1914. The three subjects which claimed most of his attention were the plankton, the submarine deposits (with the Belgian geologist abbé RENARD, 1842-1903) and the origin of coral reefs. MURRAY's Summary of results (1895) forming the last two of the fitfty volumes of the Challenger Reports (1880-1895) is of great value for the historian of oceanography.

The Challenger expedition was followed in the last quarter of the XIX^th^ century by similar undertakings organized by the United States, France, Germany, Italy, Denmark and Holland. It would have been interesting to tell briefly their stories and to state what each of these expeditions contributed to our knowledge of the ocean. Sir WILLIAM has preferred to deal with a few private undertakings: the voyages of LOUIS AGASSIZ (Neuchâtel, 1807-1873), and of his son ALEXANDER (Neuchâtel, 1835-1910), who covered more than 100,000 miles in tropical seas between 1877 and 1905; the many activities of ALBERT, Prince of Monaco (1848-1922); the foundation, in 1872, of the Naples biological station by ANTON DHORN (1840-1909). All this is very good reading. An interesting episode is the discovery, in 1895, of fragments of gigantic cuttlefishes, hitherto unknown, when they were vomitted by a sperm-whale in its agony. Upon this, the Prince became a whaler! His contributions to oceanography include the discovery of the Monaco deep near the Azores, the systematic investigation of currents by means of drift bottles, the bathymetrical chart of the seas, the organization of the Paris and Monaco institutes.

It is not necessary to indicate in detail the contents of the second part of Sir WILLIAM's book. He has done his best to win the reader's interest, leaving out technical subjects and developing the more curious ones. For example, a propos of currents, he tells the story of the tile-fish (*Lopholatilus chamaeleonticeps*) which

appeared off New England in 1879 and disappeared in the spring of 1882, when a submarine catastrophe destroyed more than a billion of them. The description of oyster- and mussel-culture in France is also very interesting. Mussel-culture is seen in its most elaborate form on the west coast of France, in the great, shallow, muddy bay known as « Anse de l'Aiguillon ». « It was established by an Irishman called WALTON, who was wrecked there in 1235 » (?) (p. 288).

The volume is completed by the « Memorandum on a proposed national expedition for the exploration of the sea » — a new Challenger expedition — which was submitted to the British Government by the Council of the B. A. A. S. in 1920 at the initiative of its president of that year, — the author. I have read the whole book with much pleasure.

G. S.

Termier, Pierre. — *A la Gloire de la Terre, souvenirs d'un géologue*, 427 p. in-8°, *Nouvelle Libr. nationale*, Paris, 1922. [15 fr.]

Réimpression d'études éparses, où l'on sent partout la passion ardente du savant pour la science à laquelle il a voué sa vie, l'admiration en laquelle il tient la nature, et la philosophie où l'a conduit « la méditation sur le Temps, familière au géologue ».

TERMIER a réuni dans ce volume qu'il consacre *A la Gloire de la Terre* un discours sur les *Sciences de la Terre* (1907), dans lequel il fait revivre deux de ses maîtres : MALLARD (1833-1894), qui « avait résolu sa vie dans l'étude de la structure des cristaux », et BÉGUYER DE CHANCOURTOIS (1820-1886), « un idéologue passionné, un cerveau toujours en ébullition »; une conférence donnée la même année sur la *Synthèse géologique des Alpes;* une autre (1911) sur les *Problèmes de la géologie tectonique de la Méditerranée occidentale;* sa fameuse conférence de 1912 sur l'*Atlantide;* une étude de 1913 sur le *Congrès géologique international du Canada;* une conférence de 1919 sur les *Grandes Enigmes de la Géologie;* une de 1920 sur *Les Océans à travers les Ages;* une méditation sur *Le Temps*, écrite également en 1920, à la suite d'une visite à la caverne d'Altamira; et trois études sur HIPPOLYTE LECHAT (1901), MARCEL BERTRAND (1908) et EDOUARD SUESS.

Dans sa notice sur HIPPOLYTE LECHAT [Cruzeilles (Haute-Savoie), 1829, août, 24 — Chambéry, 1901, janvier, 4] (pp. 29 à 45), l'un de ceux parmi les géologues qui se sont occupés des Alpes françaises, « qui ont vu le plus juste et qui ont vu juste très longtemps avant les autres », il nous fait assister aux luttes que se livrèrent,

en 1861, lors d'une réunion extraordinaire de la Société géologique de France dans les Alpes, les partisans, personnifiés par LECHAT, de l'existence dans les Alpes du houiller et du permien métamorphiques, et leurs adversaires, dont le plus notable était CHARLES LORY (1823-1889).

Avec l'éloge de MARCEL BERTRAND (Paris, 1847, juillet, 2 — 1907, février, 13), nous pénétrons (pp. 147-208) dans tous les domaines de la géologie, sauf la paléontologie et la pétrographie « où il ne voulut jamais entrer », et nous faisons avec le maître de la tectonique française des excursions dans le Jura, en Provence, dans les Alpes de Glaris, les Alpes de Savoie, les montagnes de l'Ecosse, le bassin houiller du Nord de la France, puis dans l'Afrique du Nord (1896), en Russie, etc.; à propos de l'œuvre de BERTRAND en Algérie, l'auteur rapelle que « l'intervention fortuite et momentanée de MARCEL BERTRAND dans la géologie de cette contrée a été le signal du renouvellement presque complet des idées que l'on s'en était faites ».

SARTON a consacré à SUESS et à *La Face de la Terre* une étude assez longue (*Isis*, II, 381-392, 1919), pour que je ne fasse que signaler la notice de TERMIER, parue pour la première fois en juin 1914. Mais l'auteur reproduit aussi les pages qu'il écrivait en 1918 comme *Epilogue de la Face de la Terre;* il y trace rapidement l'histoire de la géologie de 1882, année où parut le premier volume de *Das Antlitz der Erde* jusqu'au moment où sortait des presses le dernier fascicule de la traduction française, et c'est pour lui l'occasion d'un éloge d'EMMANUEL DE MARGERIE, le traducteur de ce livre dont l'influence sera « bienfaisante et illuminatrice, conseillère de travail patient et désintéressé, de réflexion audacieuse et féconde, conseillère de fraternité ».

Le livre de TERMIER est tout empreint d'une flamme ardente, bien de nature à fixer encore une âme jeune « dans l'étude spéculative, dans la recherche désintéressée, dans la contemplation de l'Univers magnifique, dans l'indicible joie de voir, de connaître, de comprendre ».

L. GUINET.

Leonard Eugen Dickson. — *History of the Theory of Numbers*, vol. III Quadratic and Higher Forms, 313 pages Carnegie Institution of Washington (N° 256), Washington, 1923

The first two volumes of this History, which have already been noticed in *Isis* (1), met with so favourable a reception as to en-

1) Vol. III, 446; vol. IV, 107.

courage the author to complete promptly the present third volume. As the author states, « it is doubtless the most important volume of the series ». Naturally also, it is somewhat different in style to the others, partaking of the nature of a connected, though necessarily highly condensed « treatise » on the subjects with which it deals. A further distinction too, one that has forced the difference just mentioned, is that in it we are concerned with general theories rather than with special problems; hence the connections between the work of successive mathematicians in the development of the theory of a special branch become of paramount importance.

Chap. I opens with a brief summary of the miscellaneous topics of which it is composed. These show the gradual development of the subject of binary quadratic forms, from the representation by FERMAT of primes in the forms $x^2 + y^2$, $x^2 + 2y^2$, etc.; its simplification (through reduction and the principle of equivalence) by the work of LAGRANGE, LEGENDRE, GAUSS, HERMITE and others; and its application at a recent date to certain special problems.

The next seven chapters deal with specific sections of the theory, such as composition of forms (Chap. III), orders and genera (Chap. IV), class-number (Chap. VI) — the latter a long chapter of particular interest and great difficulty that is written in an admirable manner.

Chap. VII traces the history of the theory of Ternary Forms, from the assertion by FERMAT that $8n - 1$ is the sum of three squares and the preliminary work of GAUSS, through the arithmetical theories of HERMITE and the work of EISENSTEIN, to the more recent work by BELL, MORDELL and HUMBERT. The chapter closes with the recommendation of a treatise (BACHMANN, 1898, a rather unusual but perfectly justifiable (one might almost say in the peculiar circumstances most desirable) proceeding in a « History » of a subject such as this, where the information has hitherto been scattered among various periodicals, some of which are unavailable to, and unreadable by, the ordinary student of the subject. This naturally leads to chapters on Quarternary and Higher Quadratic forms (with n variables) and the volume concludes with Cubic Forms in general, the special Hermitian Forms, Representation of Forms Modulo p, and Congruency Theory of Forms.

This volume, which is bound to prove of inestimable service to the mathematician engaged in research on the Theory of Numbers, has another characteristic : it will delight the soul of such as think that a history of a subject should not be a mere record of the facts of the subject, as so many so-called histories are, but a connected

tale of the development of the thought and ideas which form the backbone of the subject.

(*Manchester.*) J. M. CHILD.

Knud Faber. — *Nosography in modern internal medicine*. With an introductory note by RUFUS COLE, XIII + 222 p., 21 full page portraits. New York, HOEBER, 1923. [$ 3.75.]

The number of books on the history of medicine which have been published lately (1), is so large that the reviewer does not meet a new one with great enthusiasm. The present book appeared first in Danish (2) in 1919. It will recommend itself to many readers by the beautiful full-page portraits which illustrate it; by its brevity — it can be easily read in an evening; but chiefly by the fact that its scope is sufficiently limited to make a continuous story possible. The average history of medicine is not a history of medicine, but a fragmentary history of science from the medical standpoint. It is generally so complex as to become, even under the best circumstances, unreadable.

FABER has had the excellent idea to restrict his subject to one part only of clinical medicine, namely nosography, that is, the description of diseases.

He gives one a very lucid account of the development of nosography, showing the gradual steps which have led to our present day knowledge.

I will very briefly indicate the contents of his book. 1. SYDENHAM *and the nosologists*. For centuries physicians could not do more than describe some conspicuous symptoms or symptom-complexes. SYDENHAM tried to establish clinical descriptions of a more synthetic kind, but the applications of his method were very limited. Among other nosologists of the following century, one of the most interesting was the Montpellier physician, FRANÇOIS BOISSIER DE SAUVAGES (1731), very much admired by LINNÉ. 2. *The Paris School. Anatomic diagnosis*. PINEL, BICHAT, CORVISART, LAENNEC. The physical diagnosis introduced by the latter's stethoscopy (1819) marks the beginning of a new era which dominated the first half of the XIXth century. Nosography was then chiefly

(1) For example : HÜBOTTER, 1920 (*Isis*, IV, 369); SCHWALBE, 1920 (*Isis*, IV, 557); GARRISON, 1921 (*Isis*, IV, 554); OSLER, 1921 (*Isis*, IV, 556); SUDHOFF, 1922 (*Isis*, V, 188); MEYER-STEINEG and SUDHOFF, 1922 (*Isis*, V, 188); LIBBY, 1922 (*Isis*, V, 478).

(2) Strömninger i den nyere tids medicinske klinik med saerligt hensyn til nosografien 120 p., Copenhagen, Schultz, 1919

based upon pathological anatomy. 3. *German physiological medicine.* Another stream of thought was now furnished by the physiological work carried on largely in Germany (not exclusively there, think of CLAUDE BERNARD!). However, both the anatomical and the physiological methods of research became effective only when they were applied in conjunction with, and under the guidance of, direct clinical observation of the diseases. 4. *The bacteriological clinic.* The advent of bacteriology made possible, for the first time, the establishment of a series of clinico-pathological, instead of pure symptomatic, pictures. Clinical research was now supplied, for the first time, with a unity of aim. This is very well illustrated by the history of syphilis and of tuberculosis. The therapeutical results of this were the creation of serum- and chemotherapy. 5. *Functional diagnosis.* A development of the physiological method, its more complete harmonization with the clinical (FRERICHS and LEYDEN, 1880; CHARCOT; KUSSMAUL, OTTOMAR ROSENBACH; WILLIAM STOKES; CHRISTIAN BOHR, etc., etc.). « Functional diagnostic has directed the footsteps of the clinician, who is interested in the methods and data of physiology, back to the bedside, where his true kingdom lies. » 6. *Constitutional pathology.* This phase, a consequence of modern research on heredity, belongs almost to the XX^th^ century. The new interest in the general predisposition, in the innate constitution of the organism, expressed itself in the form of direct opposition to the doctrine of specific diseases and specific morbid causes. (HUEPPE, 1893; KRAUS's « anthropological » clinic at the Charité in Berlin, etc.).

A wise book based less on other books than on genuine clinical experience. Physicians who would loose themselves in a history of medicine of the usual type, will read Dr. FABER's account, I am sure, with great pleasure and advantage.

G. S.

Paul Masson-Oursel. — *La philosophie comparée* (Bibliothèque de philosophie contemporaine). 203 p. Paris, ALCAN, 1923.

[12 fr. 50.]

M. MASSON-OURSEL, chargé de suppléance à la Faculté des Lettres de Paris et à l'Ecole des Hautes Etudes religieuses, fut l'un des premiers amis d'*Isis;* il collabora au premier fascicule et à plusieurs autres. La plupart des ouvrages relatifs à la pensée hindoue ont été analysés par lui. Les lecteurs d'*Isis* se souviennent sans doute de son excellente Bibliographie de l'Indianisme qui parut dans le tome III, pp. 171-218, en 1920. L'ouvrage qu'il vient de nous

donner est le fruit d'études vastes autant que complexes et de longues méditations. Il est assez bref mais éminemment substantiel et suggestif.

Il est assez piquant de noter que la pensée mère en est déjà exprimée par ce THÉODORET qui fut évêque de Cyr dans la première moitié du v^e^ siècle, comme suit : Δεῖξαι ἐκ συγκρίσεως τὸ διάφορον. Ἐκ παραλλήλου θεώμενος τὸ διάφορον. Et l'auteur ajoute (p. 3) : « Ce programme, qui comporte au même titre cette contre-partie : Ἐκ παραλλήλου θεώμενος τὸ ὁμοῖον, nous le tenons pour la condition même de l'avènement de la philosophie à la positivité. » Il est étrange qu'un tel programme soit déjà énoncé au v^e^ siècle et ne reçoive un commencement d'exécution qu'au XX^e^ !

A vrai dire, ce qui n'était chez THÉODORET qu'une remarque accidentelle est devenu chez M. MASSON-OURSEL le principe d'une *méthode des sciences morales*. Il nous montre que la philosophie comporterait une objectivité du même ordre que celle des sciences de la nature, si elle s'astreignait non à construire l'esprit, mais à l'étudier dans ses productions, dont les plus caractéristiques sont les religions et les métaphysiques prises comme des faits réels, donnés dans *l'histoire*. Une telle science serait à la fois *positive* et *critique*, si elle confrontait les représentations que se firent de l'esprit et du monde les diverses civilisations, c'est-à-dire si elle devenait comparée. Cette méthode me paraît excellente et je ne reprocherai pas à l'auteur d'en avoir restreint l'application aux domaines qui lui sont les plus familiers : religion et philosophie, mais il aurait dû tout au moins indiquer son application à d'autres domaines tout à fait fondamentaux: l'art et la science. Je m'occupe depuis plusieurs années à préparer une « Introduction à l'Histoire de la Science » dans laquelle je m'efforce de mesurer le progrès accompli, siècle par siècle, non seulement en Occident, mais aussi dans l'Inde, en Chine, au Japon, et ce travail me suggère constamment des comparaisons du même genre que celles de l'auteur et qui sont également utiles pour la constitution d'une science de l'esprit. Quoi qu'il en soit, il a rendu un très grand service en insistant sur la valeur tout à fait fondamentale de l'histoire dans l'étude de l'homme, car l'homme ne se peut connaître d'une façon objective que dans le miroir du passé : « Le point de vue de l'humanité, voilà pour l'homme le point de vue absolu : ce ne fut jamais l'humanisme, mais l'insuffisante exploration de la relativité humaine qui compromit dans le passé la réalisation des sciences morales » (p. 190). Cela est fort bien dit.

L'auteur a bien fait de borner ses comparaisons à l'Occident, l'Inde et la Chine, car « leur histoire se développe en un triple synchronisme d'une parfaite continuité depuis au moins quinze cents ans avant notre ère ». Introduire d'autres peuples aurait inutilement compliqué cette esquisse préliminaire. Bien entendu, ceci l'obligeait à considérer la religion autant que la philosophie, car ce n'est que dans notre propre civilisation — résultat de deux courants distincts, judaïque et grec — que science et foi s'opposent au lieu de se mêler.

L'ouvrage est divisé en deux parties, dont la première est consacrée à l'exposition et à la discussion de la méthode comparative et la seconde à l'examen rapide de quatre exemples : la chronologie comparée des philosophies, la logique comparée, la métaphysique comparée, la psychologie comparée. Je dois me borner à considérer le premier. L'auteur nous montre que les traditions philosophiques grecque, hindoue, chinoise, débutèrent toutes trois par une période sophistique. Or, ceci éclaire notre compréhension de la sophistique. En effet, cette simultanéité implique (p. 91) « que la pensée n'opère librement qu'après avoir, par une négation systématique, fait place nette des croyances traditionelles; que ses fonctions discursives préparent ses aptitudes constructives; que l'exploitation éristique des ressources du langage prélude à la formation des concepts abstraits ». Cette période fut terminée en Grèce par SOCRATE et en Chine par CONFUCIUS. Il y succéda partout une période de dogmatisme. En Grèce, la transition fut courte, grâce au génie transcendant d'ARISTOTE qui fut « la première incarnation et l'inspiration constante de la scolastique ». En Chine, au contraire, la transition fut extrêmement lente et la scolastique néo-confucéenne, le Hsing-li, ne commença à se formuler qu'avec CHOU TUN-I au XI^e^ siècle, et ne fut vraiment solidifiée que par CHU HSI au XII^e^. Nous voyons aussi se constituer deux mouvements scolastiques dans l'Inde : une scolastique bouddhique, établie par BUDDHAGHOSA au V^e^ siècle et une scolastique brahmanique fixée par CANKARA, au IX^e^.

« Ainsi, dans les trois civilisations, une scolastique a succédé à une phase où l'activité intellectuelle était l'apanage d'une ou de plusieurs générations de sophistes. En Grèce, les deux périodes ne se séparent que par la personnalité géniale de PLATON, en qui l'esprit hellénique atteint son plus riche épanouissement, trait d'union entre le dernier, le plus grand des sophistes, SOCRATE, et le premier, le plus grand des scolastiques, ARISTOTE. Dans l'Inde, les deux phases s'interpénètrent : la dogmatique boudhique, œuvre de véritables sophistes, s'intègre dès sa formation dans un canon de structure scolastique. La Chine demeure sophistique jusqu'à l'introduc-

tion du Bouddhisme : l'allure presque socratique de l'enseignement de CONFUCIUS, les procédés de discussion des Taoïstes, LAO, LIE, TCHOUANG, attestent, malgré l'ampleur de la pensée, ce mélange d'agnosticisme et d'éristique suraiguë auquel, sous toutes les latitudes, se reconnaissent les sophistes. Le passage d'une phase à l'autre se traduit par le remplacement d'une effervescence désordonnée, riche en idées neuves, mais ignorante ou négatrice de toute discipline, par une organisation intellectuelle consciente de son but et de ses moyens, armée contre toute innovation et décidée à se maintenir dans cette stabilité que l'on rêve pour toute œuvre parfaite ».

M. MASSON-OURSEL montre avec beaucoup de finesse en quoi ces deux stades de la pensée, sophistique et scolastique, s'opposent, et en quoi aussi ils se ressemblent. L'écueil de celle-là était de ne fournir qu'une rhétorique, le danger de celle-ci est de sombrer dans le verbalisme (p. 97). « Toutes deux, en effet, prennent l'expression de la pensée, c'est-à-dire le langage, pour base de la spéculation: comment s'étonner qu'elles en demeurent prisonnières? » La Chine et l'Inde ne sont jamais sorties de la scolastique; l'Europe, au contraire, et c'est là son originalité, s'y est soustraite. Cette rupture fut entreprise par la Renaissance, mais elle fut plus longue et pénible que ses initiateurs ne l'avaient rêvé. Commencée au XVI^e^ siècle, elle n'est pas encore terminée de nos jours et les pays d'Occident sont encore encombrés d'une foule de scolastiques, conscients ou inconscients. L'auteur nous montre que ce qui caractérise essentiellement cette grande révolte de la Renaissance, ce n'est pas le goût de l'antiquité ou de la nature, mais la volonté d'individualisme, l'esprit critique (p. 99).

« Le retour à l'antiquité ne rénova la spéculation que parce que l'histoire mieux connue permit de mettre en parallèle avec la société ou la science chrétiennes une société, une science païennes pourvues d'idéaux non moins justifiables que les idéaux chrétiens; en s'hypnotisant dans la contemplation d'un âge d'or tout fictif, Hindous et Chinois se consumèrent, par contre, en regrets stériles. Le retour à la nature ne suggéra des méthodes encore inéprouvées que dans la mesure où la pensée, apprenant à se défier d'elle-même, se mit à l'école des choses pour les interroger sur leurs lois: faute d'avoir jamais fait cet aveu d'ignorance, l'Inde et la Chine, même quand elles exaltèrent la nature, ne purent secouer le vain prestige de leur science prétendue parfaite. L'Europe moderne s'est avisée que la science ne saurait être œuvre d'école; s'il s'agit non de démontrer la vérité formelle, mais de promouvoir la recherche, il n'existe que deux sources d'information : la nature et l'histoire ».

Je n'ai pas le temps de résumer les trois autres exemples, mais je veux cependant citer la conclusion du deuxième, relatif à la logique, et d'autant plus que l'auteur était admirablement préparé par ses travaux antérieurs pour le développer (p. 135).

« JOHN STUART MILL, qui rêva d'une logique empiriste et regretta de ne pas la trouver chez ARISTOTE, eût pu en découvrir maintes esquisses et plusieurs achèvements parmi les méthodes orientales. Il y aurait appris que la condition première est de ne point partir de concepts, et que l'homme responsable de la tournure qu'a prise en Occident la pensée logique, c'est SOCRATE. Cet enseignement l'eût dégagé des postulats qu'il garde en commun avec les logiciens rationalistes. Le nominalisme chinois lui eût attesté que les noms ne supposent pas nécessairement des essences idéales, comme les Idées de PLATON, mais requièrent alors des essences réelles, une nature des choses; le nominalisme indien lui eût révélé qu'un idéalisme sans idées se peut concevoir, puisqu'il existe à travers toute la tradition bouddhique. A nous autres, l'Orient apprendra que notre logique conceptuelle porte un caractère exclusivement européen; qu'ailleurs on a eu la notion de relations intelligibles autres que cette décomposition d'un tout en ses éléments, l'analyse, ou que cette composition des éléments en un tout, la synthèse; et que l'on a construit des théories du raisonnement non fondées sur des théories du jugement, des théories du jugement non fondées sur des théories du concept. La classification par genres et espèces, permettant des définitions, voilà un type grec d'intelligibilité, auquel ne se ramènent ni la classification indienne, soit par symétrie analogique, soit par moments successifs, ni la hiérarchie d'inspiration toute sociale, chère à l'esprit chinois. Une logique de ce genre ne se trouve que dans la filiation socratique; une logique de la nécessité se rencontre, mais en des sens différents, dans l'Occident et dans l'Inde : chez nous, solidaire d'une fonction d'universalité, la raison; là-bas, dépendant d'une investigation sur la structure des phénomènes; enfin une logique de l'ordre spécifie la mentalité de la Chine. Point n'était besoin d'ailleurs que l'on admît des idées au sens européen de ce mot pour que l'on pensât des contraires : les oppositions du vrai et du faux, du bien et du mal, du beau et du laid, n'impliquent en effet ni genres ni espèces. Aussi un évolutionnisme conciliant avec leur distinction quelque équivalence des contraires a-t-il pu se produire chez LIE et TCHOUANG comme chez ASANGA, comme chez HEGEL ».

Ces extraits suffisent à indiquer la fécondité de la méthode préconisée par l'auteur et sa propre habileté à s'en servir. J'ajouterai

que chaque chapitre est suivi d'une bibliographie choisie fort complète et que l'ouvrage contient un bon index.

Celui-ci aurait été encore meilleur, cependant, si les noms de la table chronologique et des bibliographies y avaient été inclus et si STUART MILL avait été classé sous son propre nom, MILL!

G. S..

William D. Weaver (editor). — *Catalogue of the* WHEELER *gift of books, pamphlets and periodicals in the Library of the American Institute of Electrical Engineers*. With introduction, descriptive and critical notes by Brother POTAMIAN (1847-), 2 vol., 504 + 475 p., illustr. New York, American Institute of Electrical Engineers, 1909.

Paul Fleury Mottelay (1841-1922). — *Bibliographical history of Electricity and Magnetism chronologically arranged...* With an introduction by SILVANUS P. THOMPSON (1851-1916) and foreword by Sir R. T. GLAZEBROOK, XX + 673 p., illustr. London, GRIFFIN, 1922. [42 s.]

The bibliography of electricity and magnetism has been studied more deeply than that of any other branch of physics. WEAVER's and MOTTELAY's works are both important contributions, which will greatly facilitate the historian's task, yet they are still very imperfect. Let us examine them .

SCHUYLER SKAATS WHEELER, a prominent American engineer and business man (born at New York in 1860), had an opportunity of acquiring the famous collection of books on electricity and magnetism collected by LATIMER CLARK (1822-1898) and said to be the most complete in existence. He eagerly seized it and presented that library (in 1901) to the American Institute of Electrical Engineers. This splendid gift brought forth the most fortunate results. It was necessary to find promptly a permanent home for it, indeed, or else the library would revert to its donator. ANDREW CARNEGIE, the most modest and intelligent of Maecenases, was still living. He thereupon decided to build the « Engineering Societies' Building » of New York, a palatial structure of which the two upper floors are devoted to library purposes. The collections housed in it form one of the best engineering libraries available to-day. Besides, CARNEGIE made possible the publication of the catalogue of the WHEELER Gift, thus increasing considerably its value. This catalogue is illustrated with a good number of portraits and of facsimiles of title-pages or typical pages of the essential items. It is

divided as follows : Volume I is entirely devoted to the main portion of the collection, 2447 items arranged in chronological order of publication, new editions or translations being mentioned immediately after the mother publication. Thus the numbers 1-5 are (briefly) :

1. VINCENTIUS BELLOVACENSIS. *Speculum naturale.* Strasburg, 1473; 2. SACROBOSCO. *Spera mundi.* Venezia, 1478 (Venice 1490); 3. ALBERTUS MAGNUS. *De anima libri III, de intellectu et intelligibili libri II.* Venice 1494; 4. PLINIUS. *Nat. hist.* Venice 1497 (English translation, 1634); 5. LEONARDUS, CAMILLUS. *De sestertio pecuniis ponderibus et mensuris antiquis.* 1510?

The weakness of such arrangement is obvious. The date of printed publication is of great importance for modern works, but to apply it to works, which appeared (and were actually published) before the invention of printing, leads to capital absurdities. Neither is it necessary to read any further to realize that the collector of those books on electricity cast a very large net indeed and let nothing go which had perchance been caught into it. I have already quoted the incunabula of the collection; p. 51 to 81 are then dealing with the XVI^th century publications; p. 81 to 149 with the XVII^th century; p. 149 to 253 with the XVIII^th century; p. 253 to 504 with the XIX^th century. Brother POTAMIAN (*alias* Dr. M. F. O'REILLY of Manhattan College, New Nork) has added many notes underlining the signification of certain items or justifying their inclusion. For example BAILEY's *Dictionarium Britannicum*, second edition, London 1736, is included because it contains « articles on electricity, gagates, lyncuricum, magnet, barometer ». It is clear that on the same principle almost any other encyclopedia or dictionary might be mentioned as well. SWAMMERDAM's *Biblia naturae*, Leydae, 1737-1738, is included for a better reason: « On p. 839, experiments on *frog's legs* are described in which the author obtained in 1658 muscular contractions by using silver and copper wires. This was more than a century before GALVANI's experiments of a similar nature » (pl. XLIX of the *Biblia* is reproduced). The second volume contains the more technical publications classified as follows : II. Excerpts from periodicals, miscellanea; III. Instructions, rules and regulations for telegraph operations. Tariffs. Codes. IV. Reports of telegraph and cable companies. V. Prospectuses of telegraph and cable companies. V. Reports of electric light, telephone and manufacturing companies. VII. Patent specification. Litigation. VIII. Parliamentary papers. Legislation. Legal. IX. Expositions. Congresses. Societies, banquets, etc. X. Trade catalogues, circulars and price lists.

XI. Periodicals. An appendix (p. 409-418) is devoted to the « Sympathetic telegraph », an imaginary magnetic telegraph, first described in print by G. B. DELLA PORTA in 1558, who had obtained the idea from the famous cardinal and humanist PIETRO BEMBO (1470-1547). This idea was simply to render telepathic communications more precise by some mechanical means. The author gives a surprisingly long bibliography of this strange subject extending from 1558 to 1881. The work is completed by an Index to authors and, in consideration of the fullness of the telegraphic collection, by an Index to telegraphic entries.

The second and more recent work is distinctly superior to the former. Some of its parts, at least, represent already a stage intermediary between bibliography and history. Another cause of superiority is the fact that it is more strictly limited to the early history of the subject. The upper limit is FARADAY, Christmas 1821, a very good choice indeed. The author has been less fortunate in the determination of his lower limits. His earliest date is 2637 B. C, HUANG-TI; then follow a series of other dates equally fanciful until we reach HOMER, a part of whose *Odyssee* (Book VIII) is quoted as evidence of some knowledge (?) of the compass. Not less than 19 pages are thus devoted to the period B. C.; then follow p. 20 to 56, dealing with the first thirteen centuries of our era (10 pages being devoted to PEREGRINUS); p. 58 to 92, with the centuries XIV to XVI (10 pages devoted to GILBERT); p. 92 to 146, with the XVII^th century; p. 147 to 339 with the XVIII^th; p. 339 to 499 with the years 1801 to 1821. Some of the accounts are very long and each is followed by abundant references. The illustrations are less numerous than in the WHEELER catalogue but more beautiful. This work is a treasure of information and is, as I said, distinctly on a higher level than the former,yet it is open to the same very serious criticism. This is obvious from its very threshold, for the frontispiece represents ST. AUGUSTINE's *City of God* from a Chantilly MS. You ask yourself: « Why? » Simply because there is an allusion to the attraction of the magnet in *De civitate Dei*, II, 4!! It is perfectly clear that such inclusiveness defeats the main purpose which is to retrace the progress of electrical and magnetical knowledge. The author was the victim of a dangerous disease which affects many collectors; they are so engrossed by their quest that it becomes gradually in their own eyes the hub of the universe. They can see nothing without trying to relate it to their hobby and they generally succeed ! When I spoke some time ago (*Isis*, V. 279) of LA TORRE's history of the womb, I said that it was absurd to rewrite the history of anatomy apropos of each organ. My crit-

icism of MOTTELAY's remarkable compilation is of the same kind, though I hasten to add that his work is infinitely superior to LA TORRE's ill-digested history, for it is clear, well-ordered, concise, and forms an excellent dictionary of the subject. It is completed by a very elaborate index embracing additional data (p. 565-673) and by five appendixes : 1. Accounts of early writers, navigators and others alluded to by GILBERT and not already disposed of (p. 501-544) ; 2. Discoveries made by GILBERT, designated in *de magnete* by large asterisks; 3. The *Philosophical Transactions* of the Royal Society, the *Philosophical Magazine and the Journal des Scavans* (a very useful description of the various series of these journals) ; 4. Additional works (1486 to 1800) wherein the reader (if he has still some curiosity left) may find allusions to electricity and magnetism; 5. The MERCATOR's projection. The just claim of the English mathematician EDWARD WRIGHT (p. 559-564).

It will be seen that MOTTELAY's work, however superior to the previous one, does not supersede it. It stops at the year 1821, and for the greater part of the XIX^th^ century the WHEELER's catalogue remains our only guide. It will be hardly possible to write the history of electrical engineering without continuous reference to it. These two great works must thus stand side by side on the shelves of every historian of physics and engineering.

MOTTELAY's work, being a step nearer to a real history than the WHEELER's catalogue, is far more readable and stimulating. It will probably induce some readers to examine the *ipsissima verba* of the great inventors — « Pioneers; O Pioneers ! »

GEORGE SARTON.

Roy L. Moodie (Associate professor of anatomy in the University of Illinois). *Paleopathology*. An introduction to the study of ancient evidences of disease. 567 (1) p., 49 fig., 118 pl. Urbana, Illinois. University Press, 1923. [$ 7.50.]

This is an important contribution to the first chapter of the history of medicine — prehistoric medicine. It is a very complete survey of all the materials which enable one to know what little can be known of diseases, their causes and their cures before

(1) The text is much shorter than this number would suggest for each plate is accompanied by 2 p. largely blank. That is, one is blank and the other contains the explanation of the plates and figures placed opposite. An extravagant but convenient disposition.

the existence of written documents. The word *before* and *prehistoric* must be given here their broadest connotation. The materials examined by the author range from the Algonkian bacteria discovered by C. D. WALCOTT in 1914 down to pre-Colombian and Inca ossements; in other words, they range from times so remote that we can not possibly imagine their remoteness down to.... yesterday. The terms prehistoric and paleopathologic are equally unsatisfactory, because the author has mixed two subjects which it would perhaps have been better to leave separated : pathological conditions 1) in the geological ages anterior to our own and 2) in prehistoric times, that is in the times which precede immediately the beginning of recorded history and, of course, as far as the New World is concerned, this may mean the XV^th^ and XVI^th^ centuries of our era. Most of the Egyptian materials investigated by WOOD JONES, ELLIOT SMITH and Sir M. A. RUFFER are not even prehistoric but belong to definite dynasties and can be satisfactorily dated.

MOODIE's work is also a valuable contribution to our knowledge of the evolution of disease, in spite of the fact that his investigations have yielded but very few positive results and that but very few questions can be answered with any definiteness. From this point of view — the history of disease — MOODIE's work forms a sort of prolegomena to AUGUST HIRSCH's *Handbuch der historisch-geographischen Pathologie* (Erlangen 1860-1864; 2. edition, Stuttgart 1881-1886).

It is very strange that the subject of « paleopathology », as defined by the author, is relatively new. The earliest observation which might be called paleopathological was made in 1774 by E.J.C. ESPER, professor at Erlangen. ESPER observed a lesion in the femur of a cave bear (*Ursus spelaeus*), probably a fracture with some callus and necrosis of the bone. Similar observations were necessarily made by almost any scientist who was called to handle a great many fossil bones, but apparently the earliest serious attempt to discuss a paleopathological topic was made by VIRCHOW, as late as 1895 (see p. 66). Thus VIRCHOW might justly be regarded as the founder of paleopathology (1).

It is even more strange that we have to wait until 1905 (that is, according to MOODIE) to find a suggestion of disease in early geological times. This suggestion was then made by MECHNIKOV in his

(1) MOODIE gives on p. 72-74 a very interesting review of paleopathologic literature in chronological order from 1774 to 1921.

work on « Immunity in infectious diseases ». The real foundation of paleopathology as a separate discipline must be ascribed to the late Sir Marc Armand Ruffer (1) whose activity along that line extended from 1909 to his death in 1917. It should be noted (2), however, that Sir Armand's activity was largely inspired by the investigations of G. Elliot Smith (1906-1917) and of Wood Jones (1908-1910). Sir Armand's chief contribution was the development of the technique for the histological study of mummies and the most brilliant fruit of his efforts was the discovery of calcified eggs of *Bilharzia haematobia* in the kidneys of two mummies of the Twentieth Dynasty (1200-1090 B. C.). So much for the history of the subject.

The large size of Moodie's book is likely to mislead many readers, or more exactly many non-readers; I mean many of the persons who will speak of the book, and place it on their shelves without taking the trouble of perusing it. It is thus well to state emphatically that our knowledge of paleopathology (3) is extremely limited. It is not simply extremely small in itself, but what is worse, we have no means of measuring the extent of our ignorance. I hasten to say that this does not disparage in any way Moodie's investigations. It is not his fault, but the fault of circumstances, if our knowledge is so terribly restricted and moreover he is very much aware of these restrictions and alludes to them more than once. Let me briefly mention them : only a small portion of each fauna is fossilized; only a small number of fossils are discovered; it is exceedingly rare that the softer parts of animal organisms are preserved, we are thus generally restricted to pathological lesions on the hard parts or to occasional impressions or casts of the soft parts; the diagnosis is thus limited to the relatively few diseases affecting the skeleton and even then to but a few symptoms of those diseases. If one remembers how hard it is very often for the physician to diagnose correctly illnesses affecting patients whom he has every opportunity to examine at leisure, one can not help being a little sceptical about the diagnosis of ailments which may have disturbed the health of a Miocene clam or of a Pliocene camel some millions of years ago ! The author is generally very cautious, but not always. The conclusion that

(1) *Isis* V, 258.

(2) Moodie fails to note it.

(3) Especially, true paleopathology as distinguished from prehistoric and Egyptian pathology.

early paleozoic animals seem immune to infectious diseases (p. 30) is unwarranted. What do we know of their infectious diseases ? The graph (opposite p. 44) showing the relative frequency of disease in the different geological periods is meaningless and ludicrous. Speculation, conscious speculation is permitted, but not to that extent !

The work is divided as follows (I quote the titles of each chapter completing them by additional information as to their contents and by a few critical remarks) : *Introduction* defining the subject. I. *Development of paleopathology.* Historical account of these studies. Tabulation of the antiquity of certain pathological processes. II. *Origin of disease.* Tabulation of all geological evidences of disease. Lesions of parasitism among Paleozoic animals. Pathology of the early fishes, amphibians and reptiles. III. *Pathological conditions among fossil plants.* This contains the substance of an additional chapter to H. H. WHETZEL's *History of Phytopathology* (see *Isis* V, 461). Galls and fungi. *Callus and fracture in fossil vertebrates.* The oldest known fracture is one of the left radius of a *Dimetrodon*, a reptile from the Permian of Texas. Carefull macroscopic and microscopic examinations of such fractures are described, also of similar lesions affecting Mesozoic and Cenozoic vertebrates. V. *Deforming arthritides in the early vertebrates.* This includes a very interesting discussion of the fossilization of blood corpuscles (p. 165-169). The first description of fossilized blood corpuscles (in a bone of *Iguanodon Bernissaertensis*) was made by A. L. L. SEITZ in 1907. In 1904 FRIEDENTHAL had discovered red blood in the body of a Siberian mammoth : the precipitin reaction of that blood was similar to that of the modern elephant. Of course, this mammoth is almost a contemporary of ours, as compared with the Belgian dinosaur. Yet SEITZ's discovery is not unplausible. VI. *Caries and alveolar osteitis among fossil vertebrates.* VII. *Chronic infections among fossil vertebrates.* Osteomyelitis in the Permian. Necroses and hyperostoses in the dinosaurs. Actinomycosis in a fossil rhinoceros. Osteomalacia in an Eocene carnivore, etc. VIII. *Parasitism among fossil animals.* Symbiosis among fossil animals. Parasitism of Carboniferous crinoids. A table shows the cases of parasitism which were theoretically possible at every period p. 287). IX. *Bacteriology of past geological ages.* The oldest bacteria are those discovered by C. D. WALCOTT in 1914 in Algonkian (Pre-Cambrian) rocks of central Montana (1). WALCOTT referred

(1) Discovery of Algonkian bacteria. *Proc. Nat. Ac. of Sci.*, t I 256, 1915.

them to the *Mitrococcus* genus; they had apparently no relation to disease. However these were not the first fossil bacteria to be described. The earliest such description was made by VAN TIEGHEM in 1879 (*Bacillus amylobacter* in the Coal Measures of Saint Etienne). Many important papers on fossil bacteria were published by BERNARD RENAULT between 1895 and 1900. Bacteria and thread-mould in the Devonian. Bacteria of the coprolites. Fossil bacteria analogous to those which produce dental caries. Bacteria in the American Permian. X. *Opisthotonos and allied phenomena among fossil vertebrates.* XI. *The extinction of races.* Why do certain groups of animals — for example those gigantic amphibians of the Triassic, the Labyrinthodonta — disappear at a certain period, and not simply in one region but everywhere? A general discussion of this problem was published by H. F. OSBORN in 1906 (*American Naturalist,* vol. 40). Disease was certainly one of the factors of extinction. A good example is furnished by the pathology of the American Mastodon.

The four last chapters are devoted to paleoanthropology. Chapter XII deals with the *Pathology of the early human races* from the *Pithecanthropus* upwards. This includes the discussion of such subjects as prehistoric syphilis (not established), prehistoric trephining, use of the cautery, amputation of fingers. XIII. *Diseases of the ancient Egyptians,* preceded by a biographical sketch of Sir M. A. RUFFER. XIV. *Disease among the Pre-Columbian Indians of North America.* Evidence of their surgical knowledge. XV. *Diseases of the ancient Peruvians.* (Our knowledge of South American paleopathology is restricted to the Inca region). Account of *Uta,* an ulcerative disease depicted on ancient Peruvian water jars. MOODIE's imposing work is completed by a bibliography and a good index.

To the criticisms already formulated, I must still add these. The author has a certain tendency to be digressive, as if he had been anxious to enlarge his subject as much as possible. I will say nothing of the many biographical notices and portraits introduced, because the notices are very short (1), and I have a weakness for portraits. But it is certainly a mistake to include a note on the measurement of geologic time, for no one ignorant of geology will care to read this book, or a chronological table of the kings of Egypt (mind you, not simply a table of the dynasties, but of all the known kings!) The author has another wrong tendency of

(1) It is, however, a fault of method to have introduced them, except of course those which are more strictly relevant, as the one devoted to RUFFER.

introducing precision where precision is altogether out of the question. For example, in his many geological tables, he assigns definite durations to the various ages (for example the Mesozoic lasted between 6 and 12,000,000 years); on p. 382 he tells us that the *Pithecanthropus erectus* is 500,000 years old, etc.

The author has already published a number of memoirs on the subject (from 1916 on) and he edited for the University of Chicago Press, in 1921, most of RUFFER's memoirs under the title *Studies in the Paleopathology of Egypt* (xx + 372 p., 71 pl.) The same press has recently published another book of his, called *The antiquity of disease* (xiv + 148 p., 1923). I have not seen it, but it presumably deals in a more popular way with the problems investigated in the book which I have just analyzed.

GEORGE SARTON.

A. A. Roback. *Behaviorism and Psychology*, 284 p., in-12°. Cambridge, Mass. University Bookstore, 1923.

Dans un recueil d'articles dédié à TITCHENER et à MCDOUGALL, M. ROBACK présente l'histoire et la critique du mouvement behaviorist qui substitue à l'étude de la conscience et de l'esprit par voie d'introspection, l'étude du comportement déterminé par l'analyse physico-chimique des mouvements des membres, des contractions des muscles, des secrétions des glandes endocrines.

Le Behaviorism a des antécédents historiques dans l'orientation de HOBBES, de COURNOT, de COMTE, d'AVENARIUS, de MACH et de SHADWORTH HODGSON. Il a été préparé par les biologistes et les physiologistes, la constitution de la psychologie animale ou comparée par UEXKÜLL, NUELL, BERR et BETHE; les travaux de PAVLOW et de BETCHEREV sur les réflexes conditionnels, l'étude des tropismes dans les organismes inférieurs, les théories motrices de l'intelligence présentées par JAMES, MÜNSTERBERG, RIBOT, DEWEY. Dès 1904, la tendance behaviorist se rencontre chez CATTELL, chez DEWEY, faisant son objet d'études de la relation chose-organisme, c'est-à-dire de la relation stimulus-réponse, chez JAMES ramenant la conscience à des ajustements intracéphaliques; chez PERRY, WOODBRIDGE, BUSH, SANTAYANA. Quant au terme « behavior », employé en 1866 par le duc d'ARGYLL, en 1869 par HUXLEY, il est appliqué en 1899 à l'organisme par H. S. JENNINGS.

Le Behaviorism désigne des attitudes d'esprit divergentes. Il est méthodologique chez YERKES, heuristique chez MCDOUGALL, biosociologique chez PARMELEE et PATON, neurologique chez MAX MEYER, pragmatiste chez BODE, génétique chez KIRKPATRICK, a-mécanique

chez BAWDEN, sélectif chez TAWNEY. Il est watsonien chez SINGER, qui identifie les mouvements des atomes et la pensée; chez HOLT, qui s'emploie à la détermination du milieu extérieur; chez WEISS, qui adjoint l'étude neurologique. Il se nuance chez K. GORDON, S. BENT RUSSEL, FROST, G. DE LAGUNA. WARREN, ANGELL ont des sympathies pour le Behaviorism. PILLSBURY, COLVIN, BAGLEY, HOLLINGWORTH, POFFENBERGER, SMITH, GUTRIE juxtaposent, malgré les protestations de WATSON, comportement, conscience et esprit. L'école de CLARKE, CALKINS, TOLMAN, KANTOR, KEMPF, présentent d'autres formes encore.

Ainsi *behavior* ne le cède en rien pour l'ambiguïté et le vague à *conscience*. C'est que behavior s'emploie aussi bien pour désigner l'activité des protéines, des atomes, des électrons que pour désigner des processus organiques identifiés arbitrairement par MAX MEYER avec les processus neuraux du système nerveux, par WATSON avec l'action des muscles et des glandes, par HOLT avec des mouvements plus considérables et le jeu des membres. Y a-t-il alors d'autre différence entre le substitut de la psychologie et la physiologie, sinon que le premier étudie des organes et des fonctions pris séparément au lieu de comprendre l'ensemble des réactions organiques?

De la sorte, le Behaviorism altère les aspects de la vie mentale, se prête à des pétitions de principes, à des vices de raisonnement, des confusions et des simplifications arbitraires. Il se prive de l'appui que confère à la psychologie la philosophie. Est-il mécaniste? Est-il moniste, comme le pense WEISS? Paralléliste, comme le pense DUNLAP? Ni l'un ni l'autre. Encore que la philosophie vienne à lui, il demeure, par matérialisme et par logique, étranger à toute métaphysique.

Aussi, malgré HOLT, WATSON, S. C. PEPPER, W. R. WELLS, W. S. TAYLOR, H. WOODROW, WIEMANN, PERRY, le Behaviorism ne saurait transformer l'éthique, la jurisprudence, la psychiâtrie, l'étude de la religion, la conception de l'intelligence, l'étude des faits sociaux au point de rendre inopérants les concepts de la psychologie traditionnelle.

Les besoins de notre époque, les sympathies des pragmatistes, la combativité des psychologues, ont pu assurer quelque vogue au Behaviorism. L'esprit européen ne saurait accueillir cette doctrine matérialiste, limitée aux Etats-Unis, discutée seulement en Angleterre, vouée à la faillite par un manque de profondeur, de perspective et de résultats. Elle a bénéficié de la crise traversée, au cours des dix dernières années, par la psychologie américaine. C'est qu'une mésinterprétation discréditait injustement la psycho-

logie introspective. La psychologie ne saurait méconnaître la connexion des faits psychologiques avec les faits physiques, chimiques, biologiques et physiologiques. Mais il ne s'en suit pas qu'elle doive comprendre simultanément, sous le terme de behavior, la conscience et l'activité motrice; il ne s'en suit pas qu'elle doive oublier sa base : l'introspection; son but : l'établissement ou la confirmation de lois générales; son domaine : les sciences qui étudient l'homme lui-même : « We must start, however, with mental phenomena and must strive to connect them whereever they make their appearance, whether in the laboratory, at home, in the theatre, or in church. » (p. 226).

Il ne convient pas ici de départager les adversaires et partisans du Behaviorism, mais seulement de reconnaître que cette étude, accompagnée d'une classification des écoles behavioristes et d'une bibliographie, constitue une importante contribution à l'histoire de la psychologie contemporaine aux Etats-Unis. Elle marque bien l'état d'inquiétude attesté en Europe après 1870, aux Etats-Unis après 1890, par la multiplicité des techniques constituée à l'écart des grandes idées synthétiques qui faisaient la force du Positivisme et de l'Hégélianisme. Mais il semble qu'on doive comprendre parmi les antécédents historiques du Behaviorism les travaux de BINET et la psychologie du comportement telle qu'elle fut comprise et appliquée en France par PIÉRON dans son étude de la mémoire. Mais il semble que l'article de WATSON paru en 1913, « Psychology as the Behaviorist View », et ses travaux ultérieurs font époque dans la psychologie américaine : ils rendent le terme « comportement » inséparable de méthodes déterminées, inséparable de doctrines déterminées de l'émotion, de l'instinct, du raisonnement. Dans son sens strict, le Behaviorism apparaît alors comme la prise de conscience de tendances positives demeurées assez confuses pour ne pas sembler incompatibles avec des convictions métaphysiques, comme la systématisation d'un goût pour l'expérience qui ne parvenait pas à se fixer. Comme telle, quelle que soit la valeur intrinsèque qu'on lui accorde, il n'a pu manquer d'exercer une profonde répercussion sur la psychologie et la philosophie américaines. S'il passe pour ne pas pouvoir se répandre en dehors des Etats-Unis, c'est que la persistance des habitudes mentales oppose aux conceptions scientifiques nouvelles une force d'inertie et retarde les échanges d'idées. Il a fallu une dizaine d'années pour que la théorie d'EINSTEIN et la théorie de FREUD se répandent dans le monde savant et fassent l'objet de discussions internationales. Le symposion du meeting d'Oxford de 1920 sur la réduction de la pensée aux mouvements du larynx, la discussion des applications philosophiques du Beha-

viorism exposées par R. B. PERRY à la séance de la Société française de Philosophie du 26 janvier 1922 attestent cependant sa diffusion naissante.

(*Paris.*) RAYMOND LENOIR.

Friedrich Dannemann. — *Die Naturwisschenschaften in ihrer Entwicklung und in ihrem Zusammenhange*. Zweite Auflage. III. Band *Das Emporblühen der modernen Naturwissenschaften bis zur Aufstellung des Energieprinzipes*, XII + 432 S., 65 Abb., 1 Bildnis [GAUSS]. Leipzig, ENGELMANN, 1922. [12.50 Swiss francs.]

The first edition of this volume appeared in 1911 and I reviewed it in the *Revue générale des Sciences*, t. 23, 202, 1912 (1). The new edition is not essentially different from the first, but many corrections and short additions have been made by the author and also by E. WIEDEMANN, E. O. v. LIPPMANN and J. WÜRSCHMIDT. It contains 432 pages and 65 figures against 400 pages and 60 figures of the first edition. The fact that the author has become scientific associate of the *Deutsches Museum* in Munich has enabled him to improve his understanding of many points.

This volume deals with the period extending roughly from the middle of the XVIII^th^ century to the foundation of thermodynamics. It is as good as vol. 2 and far superior to vol. 1. It is hardly possible to criticise this work in detail, but is worth while to give a list of the main chapters with a few additional data.

1. Science and history (introductory chapter). 2. First electrical theories. 3. Practical and theoretical progress in the knowledge of heat. 4. Description of nature under the influence of LINNÉ's natural system. 5. Extension of physical methods to the field of plant physiology. 6. Development of the knowledge of plant sexuality. 7. Progress of zoology in the XVIII^th^ century. 8. New mathematics and its relation to natural sciences (this chapter is almost worthless, it deals with WOLF, LOMONOSOV, MONGE, PONCELET, STEINER, GAUSS, LOBACHEVSKII, ABEL, FOURIER, PFAFF, CAUCHY, JACOBI, DIRICHLET, etc. All that in 22 pages !) (2) 9. Che-

(1) For other parts of DANNEMANN's history see *Isis*, II, 218; IV, 110, 563; V, 198.

(2) Reading such a chapter gives one the impression that DANNEMANN's History was primarily conceived as a framework for the OSTWALD's *Klassiker*; as if he had been intrusted the task, so many booklets of that collection being given, to write a History wherein they would duly appear. I may be wrong, but this seems the simplest explanation of DANNEMANN's extraordinary selection and grouping.

mistry from BOYLE to LAVOISIER. 10. Introduction of quantitative methods into chemistry. 11. Atomic theory. 12. Galvanism (VOLTA). 13. Foundation of electrochemistry. 14. Fundamental phenomena of electromagnetism and electrodynamics. 15. Thermoelectricity. 16. Development of astronomy by LAPLACE and HERSCHEL. 17. Fundamentals of the mechanical theory of heat (RUMFORD, DAVY; thermic properties of gases). 18. Progress of optics and triumph of the wave theory. 19. Beginning of more intimate relations between chemistry and physics (GAY-LUSSAC, WILLIAM HENRY, DALTON). 20. Mathematical applications to the natural sciences (FOURIER, GAUSS, LAPLACE, LAGRANGE, GREEN, WEBER, SOLDNER). 21. Beginning of physical geography (HUMBOLDT, BUCH). 22. Physico-chemical development of mineralogy. 23. Establishment of a natural classification of plants (A. L. DE JUSSIEU, GAERTNER, A. P. DE CANDOLLE, GOETHE). 24. Physico-chemical development of plant physiology. 25. Progress of zoology and its fusion with comparative anatomy. 26. Geology and paleontology under the influence of catastrophism. 27. Progress of the theory of evolution (WOLFF, PANDER, K. E. v. BAER, RATHKE).

This brief summary is sufficient to indicate the richness of this volume, a richness which is also a poverty, for it precludes long developments and obliges the author to jump incessantly from one subject to another without leaving one the opportunity to deepen any. Taking these limitations into account, DANNEMANN'S book is very good and, as I said in 1912, it has one inappreciable quality, that is, to be alone of its kind.

G. S.

Edmund O. von Lippmann. — *Beiträge zur Geschichte der Naturwissenschaften und Technik*, VIII + 314 p. Berlin, SPRINGER, 1923.

The activity of Dr. E. O. VON LIPPMANN, director of the sugar refinery Halle, in Halle a. S., is almost uncanny. Two recent works of his have been reviewed in *Isis;* his monumental history of alchemy (1919, see *Isis*, III, 302-305), and his chronological table of organic chemistry (1921, see *Isis*, IV, 548). Before that he had already produced many important works. Let me quote them briefly : *Geschichte des Zuckers, seit den ältesten Zeiten, bis zum Beginne der Rübenzuckerfabrikation*, 474 p., 1890. *Die Entwicklung der Deutschen Zuckerindustrie von 1850 bis 1900*, 341 p., 1900. *Die Chemie der Zuckerarten*, 3. Aufl., 2 vol., 2004 p., 1904. *Die beiden Grundschriften der Rübenzuckerfabrikation*, A. S. MARGGRAF (1747) und F. C. ACHARD (1803). Neuausgabe mit Anmerkungen, 72 p. (OSTWALD'S *Klassiker*, 159), 1907.

The volume now under review is the third of a series wherein the author collects from time to time his shorter memoirs. The first two were entitled *Abhandlungen und Vorträge zur Geschichte der Naturwissenschaften.* The first appeared in 1906 (Leipzig, VEIT; XII + 590 p.); it contained 32 papers; the second in 1913 (*ibidem;* X + 492 p.); it contained 36 papers. The third volume contains also 36 papers; it is fully as large as the two former, though thinner, for the text is much closer.

In view of the great value of Dr. VON LIPPMANN's investigations, I give below a complete list of papers included in this third volume. For each paper, I add the date of its original publication, and I refer to the page of *Isis* where a brief analysis (1) of it may be found.

Antiquity. Early alchemy.

1. Chemische Papyri des 3. Jahrhundertes (p. 1-27), 1913 (*Isis*, V, 492).
2. Ueber chemische Papyri des 3. Jahrhundertes n. Chr. (p. 28-32) 1917 (*Isis*, III, 320; IV, 523).
3. Zur Geschichte der Alchemie (p. 33-43), 1922 (*Isis*, V. 545).
4. Noch einmal « Caput mortuum » (p. 43-46), 1921 (*Isis*, IV, 441).
5. Ueber das erste Vorkommen des Namens Chemie (p. 46-50), 1914 (*Isis*, III, 321).
6. Zur Geschichte der Volumgewichts-Ermittlung (p. 50-53), 1915 (*Isis*, III, 366).
7. Geschichtlicher Beitrag zur Erkenntnis der Verbrennungsvorgänge (p. 53-55), 1920 (*Isis*, IV, 129).

II. *Alcohol, etc.*

8. Zur Geschichte der Destillation und des Alkohols (p. 56-59), 1913 (*Isis*, V, 544).
9. Beiträge zur Geschichte des Alkohols (p. 60-107), 1913 (*Isis*, V, 544).
10. Neue Beiträge zur Geschichte des Alkohols (p. 107-123), 1917 (*Isis*, III, 323).
11. Zur Geschichte des Alkohols (p. 123-124), 1920 (*Isis*, IV, 135).
12. Zur Geschichte des Alkohols (p. 124-126), 1922 (*Isis*, IV, 590).
13. Zur Geschichte des ununterbrochenen Kühlung bei der Destillation (p. 127-136), 1915 (*Isis*, V, 507).
14. Verwendung des Petroleums im frühen Mittelalter (p. 136-139), 1914 (*Isis*, III, 354).

(1) Or at least an explanation of its title.

III. *Chemistry in the mediaeval books of recipes.*

15. Chemisches und Technologisches aus kunstgeschichtlichen Quellenschriften. I. HERAKLIUS (p. 140-157), 1916 (*Isis*, III, 354).
16. Chemisches und Technologisches aus kunstgeschichtlichen Quellenschriften. II. THEOPHILUS PRESBYTER (p. 158-174) 1917 (*Isis*, V, 497).
17. Das Sammelbuch des VITALIS DE FURNO und seine Bedeutung für die Geschichte der Chemie (p. 175-192), 1922 (*Isis*, IV, 587).
18. Chemisches und Technologisches bei DANTE (p. 192-197), 1921 (*Isis*, V, 503).
19. PETRARCA über die Alchemie (p. 197-200), 1913 (*Isis*, V, 503).
20. Technologisches aus dem « Mittelalterlichen Hausbuch » (1480?) (p. 200-210), 1922 (*Isis*, V, 220).

IV. *Varia* (see also n° 14).

21. Zur Geschichte des diabetischen Zuckers (p. 211-213), 1920 (*Isis*, V, 556).
22. Diamant und Bocksblut; ein Beitrag zur Volkskunde (p. 213-227), 1921 (*Isis*, IV, 216).

V. *Later alchemy.*

23. Ueber das Zeitalter der Alchemisten J. J. und J. HOLLANDUS (p. 228-229), 1916 (*Isis*, III, 333).
24. Ueber die unter dem Namen der « HOLLANDI » bekannten Alchemisten (p. 229-250), 1919 (*Isis*, III, 333).
25. Der Stein der Weisen und Homunculus, zwei alchemistische Probleme in GOETHE's *Faust* (p. 251-255), 1920 (*Isis*, V, 544).

VI. *Sugar* (see also n° 21).

26. Einige Mitteilungen über die mittelalterliche Zuckerindustrie (p. 256-263), 1917 (*Isis*, III, 352).
27. Zucker-Monopole im Mittelalter (p. 263-265), 1921 (*Isis*, IV, 184).
28. Ein Brief ACHARDS (p. 266-268), 1914 (*Isis*, III, 342; V, 515).
29. Ein Brief ACHARDS (p. 268-270), 1919 (*Isis*, V. 515).
30. Aus dem Briefwechsel ACHARDS (p. 270-275), 1919 (*Isis*, V, 515).
31. GOETHE und die Zuckerfabrikation (p. 275-281), 1919 (*Isis*, V, 519).
32. Zur Geschichte des Vakuum-Apparates (p. 281-284), 1916 (*Isis*, V, 521).

33. Die Entwicklung der Zukerindustrie 1888-1913 (p. 284-294), 1913 (*Isis*, V, 520).

VII. *Nineteenth Century.*

34. Ueber den Namen Berzelius (p. 295), 1917 (*Isis*, V, 520).
35. Ein kleiner Beitrag zur Liebig-Biographie (p. 295-296), 1918 (*Isis*, V, 520).
36. Zum hundertjährigen Geburtstage Robert Mayers (p. 296-302), 1914 (*Isis*, V, 521).

It is strange that the author did not include in this collection his paper on Thaddaeus Florentinus über den Weingeist, 1914 (*Isis*, III, 325).

G. S.

Science and Civilization. — Essays arranged and edited by F. S. Marvin (The unity series, 6). 350 p. Oxford University Press, American Branch, New York, 1923. [$ 4.20.]

I have already spoken (*Isis*, II, 425-427) of the « Unity History School » organized by Francis Sydney Marvin in August 1915 at Woodbrooke, the educational settlement of the Society of Friends, some four miles out of Birmingham. The subject of that teaching was « The Unity of Western Civlization ». A second course was held in 1915 dealing with « Progress and History »; a third in 1919, with « Recent Developments in European Thought »; a fourth in 1920, with « The Evolution of World Peace »; a fifth in 1921, with « Western Races and the World ». Each of these courses has been published by the Oxford University Press.

The sixth dealing with « Science and Civilization » is of special interest to us. « It was thought well in this course to attempt a more analytical line of approach and to consider in some detail one of the leading threads which have tended progressively to bind humanity together in historic times. Science was obviously the first to select » (1). Of the twelve lectures reprinted in this volume, the first six are mainly historical, the other six consider the relation of modern science to the contemporary social evolution. J. L. Myres spoke of « The beginnings of Science »; Charles Singer of « Ancient medicine » and later of « The dark ages and the dawn »; Arthur Platt « of « Biological and geological knowl-

(1) Art, the next. The Unity History School of this summer (1923) was held in Vienna and devoted to art.

edge in antiquity »; J. L. E. DREYER of « Greek mathematics and astronomy »; A. N. WHITEHEAD of « The first physical synthesis ». These lectures are certainly interesting and full of suggestion, but it is not for me to judge them. They are not contributions to knowledge in spite of many felicitous remarks and comparisons; but they make excellent propaganda for the history of science. That is, if the audience responded to them. I have no means of guessing, because I do not know how the Woodbrooke audiences are recruited and because it is not possible to deduct it from these fairly heterogeneous lectures. At any rate those who listened to them or who read them will have some idea of the development of science and may be induced to obtain more information.

The second half of the volume interested me more. It is much easier, of course, to discuss the value of science from a certain angle, within an hour's time, than to outline its history. CECIL H. DESCH spoke of « Science in the industrial revolution », explaining the dark sides of what PATRICK GEDDES has so happily called the Palaeotechnic Age, that is, the period inaugurated by the industrial revolution and which seems to have already reached its climax : natural resources are squandered and social good subordinated to private gain. In the Neotechnic Age of which we may live to see the dawn, these conditions will be reversed. J. ARTHUR THOMSON's essay on « The influence of Darwinism on thought and life » is truly excellent. Then followed A. E. HEATH's lecture on « Science and education » and F. G. CROOKSHANK's on « Science and Health ». I have read with special pleasure JULIAN S. HUXLEY's account of « Science and religion » though the author, I am sure, could have improved it by simplifying and shortening it. The conclusions of the meeting were stated in a final lecture by MARVIN. Let me quote his final words : « Greater unity, greater knowledge, and greater power, gained by the application of knowledge, — these leading aspects of man's social evolution may be gathered in one expression as the growth of the human spirit.

« We have been following the function of science in determining this evolution, and we conclude that, through science and the organization which it has occasioned, our modern civilization has reached a state of extension and stability which is a new and supremely important factor for the future. We have to see to it, that this more closely-knit and richer social spirit is adequately reflected in fuller and nobler personalities. This is the task of education, and herein our law of progress points an ideal for the future as well as summing up the achievements of the past ».

I subscribe to these conclusions though my conception of science is not entirely the same as the author's. Instead of repeating, as he does, COMTE's motto « Savoir afin de prévoir » (which might be as well the motto of a financier), I prefer to say : « Savoir afin d'aimer ». Science is not simply a technique; it is a religion. One should study as one prays.

G. S.

Maxwell, Joseph — *La Magie*. 252 p., in-12 (Biblioth. de Philos. scientif.). E. FLAMMARION, Paris, 1922. [7 fr 50.]

« L'importance de la magie comme phénomène sociologique n'est pas contestée, quoique sa réalité objective le soit. C'est une contradiction. La réalité de la magie peut seule expliquer sa persistance, sous des formes à peu près identiques dans toutes les sociétés. » La magie, phénomène social, puisqu'il varie avec l'état social, correspond, non pas à des fonctions physiologiques, mais à des fonctions psychologiques collectives, et son origine primordiale doit être cherchée dans une fonction psychologique individuelle. Malgré ses rapports étroits avec la religion dans les civilisations primitives, elle en diffère en ce que, tandis que l'acte religieux est une prière, l'acte magique est l'expression d'une volonté forte, tendant à la subjugation d'êtres surnaturels, ou à la domination des forces naturelles. Née sans doute avec le sentiment religieux dont l'origine lointaine est la crainte de l'inconnu, se confondant peut-être avec la religion au début, elle s'en écarte en devenant la forme active du sentiment religieux : la résistance de l'homme aux actions défavorables du milieu extérieur. — De même, la magie naturelle, où l'homme veut agir directement sur les forces cosmiques, et qui est l'origine de la science, et la magie surnaturelle, ou évocatoire, dans laquelle on fait appel à un être surnaturel, se confondent au début. — Ces hypothèses préliminaires posées, MAXWELL passe en revue, à leur lumière, la magie évocatoire, ses procédés, et la « force magique », la magie naturelle, la magie naturelle psychologique (la conscience organique = subconscient, est le siège des phénomènes magiques); il étudie ensuite l'évolution de la magie (surtout dans le monde occidental), ses formes modernes (théosophie, spiritisme, etc.), et sa position vis-à-vis de la psychologie, ou plutôt des sciences psychiques (dont il a d'ailleurs fait l'objet d'une étude antérieure : *les Phénomènes psychiques*, ALCAN, 4e éd., 1909) ou métapsychiques. Conclusion : l'étude de la magie « nous conduit à considérer l'être humain comme une entité dont l'évolution n'est pas terminée, dont les puissances ne sont pas toutes

développées ». — Chemin faisant, l'auteur a naturellement fait une étude de la magie, dans l'espace et dans le temps; cette étude est malheureusement trop dispersée et donc un peu confuse.

L. G.

Lt. Commander Rupert T. Gould, R. N. (retired). — *The Marine Chronometer.* Its history and development. With a foreword by Sir FRANK W. DYSON, Astronomer Royal. XVI + 287 p., 39 pl., 85 fig., London. J. D. POTTER, Admiralty Agent for Charts, 1923.

This excellent work is not simply a history of the chronometer, but, as we might expect, a history of the determination of longitude at sea. The general problem of determining longitudes is of course as old as geography (1); it was as difficult to solve it as it was easy to determine the latitude. But the determination of longitude at sea was considerably more difficult. This particular problem became urgent, when COLUMBUS opened the age of oceanic navigation. He himself was absolutely helpless about it. The best he could do was to keep his « dead reckoning », as carefully as possible, making such allowances as he thought fit for leeway, tide, current, variation of the compass, errors in estimating his speed, bad steering, etc (2).

The situation did not materially improve until more than two and a half centuries later. As late as 1741, Commodore ANSON, a sailor of long experience, spent more than a month endeavoring to round Cape Horn to the westward and found after long efforts that owing to an unexpected easterly current, he was still on the eastern side of it!

It is interesting to note that it was COLUMBUS himself, who by his discovery of the variation of the compass (1492), suggested the earliest method to solve this great problem. This method was pursued until the end of the XVII^th^ century; but though this increased our knowledge of terrestrial magnetism and was helpful to seamen to that extent, it did not achieve the main purpose. Leaving aside

(1) A brief account of its early history will be found in J. K. WRIGHT. The knowledge of latitudes and longitudes in the Middle Ages. *Isis*, V, 75-98, 1923. For the Muslim solution see also CARL SCHOY Aus der astronomischen Geographie der Araber. *Ibidem*, 51-74 (translation of a text of AL-BIRÛNÎ, with commentary).

(2) It is astonishing that he did so well. Read apropos of this the Note on the navigation of COLUMBUS by the Earl of DUNRAVEN appended to FILSON YOUNG's COLUMBUS. London 1906.

other methods, which were unreasonable or unpractical, the two only ones which could be used, originated at about the same time, — the first half of the XVI^th century — I mean the method by lunar distances, and that depending on the use of a timekeeper.

The observation of lunar distances, enabling one to compare one's local time with a standard time, was first suggested by JOHANN WERNER of Nuremberg in 1514. WILLIAM BAFFIN attempted to use it in 1515. A similar method, though utterly impracticable, was proposed in 1610 by GALILEO : it was based on the observation of the eclipses of Jupiter's satellites.

On the other hand, the use of a timekeeper was recommended as early as 1530, by the Dutch mathematician GEMMA FRISIUS. It would have been difficult to make this recommendation must earlier, for portable timekeepers (other than clepsydras) date only from the beginning of the XVI^th century; the so-called « eggs of Nuremberg » did not appear even until 1540 (1).

Both methods were easy to recommend but difficult to apply. The first implied the existence of good lunar tables, the second of reliable timekeepers. Yet something must be done. Sailors of many nations, stimulated by an intense rivalry, were now plowing the seven oceans in every direction. Accurate scientific navigation remained utterly impossible as long as longitude could not be determinated as well as latitude. A proof that the need was felt, if not by the reactionary sailors themselves, at least by the landsmen, is that as early as 1598, PHILIP III of Spain offered a considerable reward to the « discoverer of the longitude ». This was followed by many other offers of the same kind, culminating in 1714 by one of the British Government amounting to £20,000. Two years later, the Government of France offered a prize of 100,000 livres. Such offers gave incidentally a tremendous encouragement to cranks and quacks of all kinds. Nothing shows more clearly the absurd ideas entertained about this mysterious longitude than an entry like this (one of many others of the same kind) found by the author in the minutes of the Board of Longitude (1) (p. 15) : « A letter was read from Dr. WOEMEN, a native of Saxony, acquainting the Board that he can express π and the ratio of 1 to $\sqrt{2}$ in integrals, and that this comprehends the discovery of the Longitude. He was informed that the Board do not receive proposals of this nature ». And mark, the date of that entry is 1796!

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(1) F. M. FELDHAUS. *Die Technik*, 1914, 1229.

(2) The *Board of Longitude* existed from 1713 to 1828. The *Bureau des Longitudes* was not founded until 1795 but is still extant.

But to come back to our main inquiry. It may be worth while to retrace briefly the history of both methods. The observation of lunar distances was, naturally, more popular at first, for it gave more hope. Its successful application was partly a matter of patience, while the possibility of making reliable seagoing timekeepers remained for a long time extremely doubtful (1). Thus it is largely the wish to develop the first method which led to the creation, in 1675, of the Greenwich Observatory (« for rectifying the tables of the motions of the heavens and the places of the fixed stars so as to find the so-much desired longitude of places for perfecting the art of navigation »). The study of the Moon's motions took a considerable time however, and we have to wait until 1753 to obtain tolerable tables, namely those computed by TOBIAS MAYER (1728-1762). His widow received in 1765 a prize of £3 000 from the British Government (2). In 1763, the Astronomer Royal MASKELYNE (1732-1811) published the *British Mariner's Guide*, and four years later, the first edition of the *Nautical Almanac*. Both works contained tables for the application of this method. It is typical of the conservativeness of that venerable *Almanac* that lunar distances continued to be published in it until 1907, though they had long ceased to be used. To complete this sketch I might say that the sextant had been discovered in 1701 by NEWTON, and introduced some thirty years later by JOHN HADLEY. The sextant is of course mainly used for the determination of latitudes, but it was exceedingly useful also for the observation of lunar distances (and for the same reason, its relative independence from the ship's motions). Thus by the third quarter of the XVIIIth century, the method of lunar distances was sufficiently developed to be applied by mariners. It was not very accurate however and the computations involved by it consumed many *hours*. With far better tables than were then available, one would have to take great pains to be correct within twenty miles.

Happily, the second method, though less promising, proved to be the best and the present book is chiefly devoted to a study of its progress. A short but sufficient account of the Nuremberg eggs

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(1) This will be realized better if one remembers that one minute of time corresponds to 15 minutes of longitude. Thus to know one's longitude at the end of a six week's voyage within half a degree (a poor approximation) the error of the timekeeper should not amount to more than three seconds a day.

(2) An English edition of his Solar and Lunar Tables appeared in 1770. It is shocking that Comm. GOULD does not even quote MAYER ! — Another prize was given to EULER. The merit of mere theorists was thus fully recognized.

(including a technical discussion) introduces the subject; then follow a few pages dealing with HOOKE's ideas, and some more, with HUYGENS's fundamental discovery (1658) and the work of his followers. All that might be called the prehistory of the chronometer, the history of the instruments which preceded its own creation. Chapter III and IV introduce the main hero of the story, JOHN HARRISON, born at Foulby, in the parish of Wragby, Yorkshire, May 1693, and brought up to follow his father's trade, carpentry. His first marine timekeeper was begun in 1728 and completed in 1735; the second, begun in 1737 was completed in 1739; the third, begun in 1740, was not completed until 17 years later, in 1757. These three timekeepers were heavy instruments; for example, the third weighed 66 lbs; and its case and gimballing 35 lbs. Number 4 was of an entirely different type. It resembles an enormous silver pair-case watch, about 5 inches in diameter; it was not suspended in gimbals. « By reason alike of its beauty, its accuracy and its historical interest, it must take pride of place as the most famous chronometer that ever has been or ever will be made ». It was finished in 1759 and tried at sea (together with number 3) by order of the Board of Longitude in 1761. The trial was very successful, but the Board hesitated to grant the award. A new trial was made in 1764 with number 4 (after some improvement had been brought to it). It is interesting to note that one of the occasional observers was MASKELYNE, who had then just published his « British Mariner's Guide » and was, of course, the champion of the rival method of lunar distances. The result of the second trial was amazing. During a journey of five months (from Portsmouth to Jamaica and back) the average error of number 4 was less than a tenth of a second per day. It was impossible to doubt HARRISON's victory any longer. The award was made to him in 1765, but was not entirely paid, after much bickering, until 1772. A fifth and last timepiece was built by HARRISON between 1767 and 1770. Number 5 was very similar to number 4; a trial of 10 weeks at the King's private observatory at Kew revealed a *total* error of only 4 1/2 seconds. « Longitude HARRISON » died on March 24, 1776. He was buried in the cemetery of St. John's Church, Hampstead. His tomb was repaired in 1879 by the Clockmakers' Company. His son WILLIAM who was his collaborator, at least since the making of number 4, died in 1815.

The description of HARRISON's machines is very satisfactory and completed by excellent photographs and drawings. The author was extraordinarily well prepared to make this account, for he

cleaned numbers 1 and 4, and re-assembled them and restored number 4 to going order (1).

It is curious that both methods of determining longitude matured at once : MASKELYNE's *Guide* dates of 1763 (2) and the two decisive trials of HARRISON's chronometer respectively of 1761 and 1764. The superiority of the chronometer upon the « lunars » was immense, but we must not forget that it took a long time before chronometers could be produced in sufficient quantity, and until about 1825 and even later a great number of ships were necessarily deprived of them.

The following four chapters (V to VIII) are devoted to the other great pioneers who completed HARRISON's task. Before saying a few words of each of them, I must express the regret that the author did not see fit to bring the personality of PIERRE LE ROY into greater relief. To be sure, he gives full credit to him, but the mere fact of having left him among other Epigoni is an injustice. PIERRE LE ROY and JOHN HARRISON must be placed on exactly the same footing. HARRISON was the first to produce a good marine timekeeper (1759), but PIERRE LE ROY was the real father of the modern chronometer (1766). Comm. GOULD makes a technical analysis of the activities of the following :

1. LARCUM KENDALL (born at Charlbury, Oxford, 1721; died 1795). Instructed by HARRISON, he completed in 1770 a duplicate of number 4, superior to the original, and used by Capt. COOK on his second and third voyages.

2. THOMAS MUDGE (b. Exeter, 1715; d. 1794). He invented in 1754 the lever escapement used even to this day in every pocket watch. For chronometers he devised (c. 1774) the constant-force escapement, perfect but too complicated.

3. PIERRE LE ROY (b. Paris, 1717; d. Vitry near Paris, 1785). He completed in 1766 a timekeeper which may be called the first modern chronometer . He had seen the first three timekeepers of HARRISON, but not number 4, and at any rate, his work is entirely original. Says Comm. GOULD (p. 86, 90) :

« Nothing can rob HARRISON of the glory of having been the first man to produce a satisfactory marine timekeeper, one, too, which

(1) No. 1, 2, 3, 4 are kept at the Royal Observatory, Greenwich ; No. 5 is in the Clockmakers Company Museum. Two important Mss. of JOHN HARRISON were sold recently by Messrs SOTHERAN. Information as to their present whereabouts would be appreciated.

(2) The indications given in the *Guide* were hardly sufficient however The earliest lunar tables of sufficient extent and accuracy for navigation were those of the first *Nautical Almanac*, 1767.

was of permanent usefulness, and which could be duplicated as often as necessary. But number 4, in spite of its fine performance and beautiful mechanism, cannot be compared, for efficiency and design, with LE ROY's wonderful machine. The Frenchman, who was but little indebted to his predecessors, and not at all to his contemporaries, evolved, by sheer force of genius, a timekeeper which contains all the essential mechanism of the modern chronometer... HARRISON took the escapement, balance and general arrangement of the ordinary watch of his day, and by fitting a remontoire and maintainer, an automatic regulator and diamond pallets, aided by high-numbered wheels and pinions and lavish jewelling (1), he compelled it to become an efficient timekeeper. LE ROY attacked the problem from an entirely different standpoint, and obtained his results, not by nullifying defects, but by eliminating them. The difference in their machines is fundamental : HARRISON built a wonderful house on the sand, but LE ROY dug down to the rock. »

LE ROY invented the compensation balance and also the first detached chronometer escapement, both in 1765. He was also the first to devise a method of obtaining an isochronous balance spring. He explained his discoveries with perfect lucidity in his *Mémoire sur la meilleure manière de mesurer le temps en mer*, presented to the « Académie des Sciences » in 1769 and published at Paris 1770.

4. FERDINAND BERTHOUD (b. Plancemont, Neuchâtel, 1729; fl. in France; d. Groslay, Montmorency, 1807). Not a man of genius like LE ROY, but an horologist of extraordinary ability, who made a great number of experiments and minor discoveries and wrote a series of ponderous works on his art, which are of great value for the historian (*Essai sur l'horlogerie*, 1763; *Traité des horloges marines*, 1773; *Histoire de la mesure du Temps*, 2 vol., 1802, etc.)

5. LOUIS BERTHOUD, his nephew (1750-1813), received a prize from the Institut in 1799 for a chronometer showing decimal (or « republican ») time. He was a very distinguished horologist.

6. JOHN ARNOLD (b. Bodmin, Cornwall, 1736; d. 1799). He and his great rival EARNSHAW were the first to manufacture chronometers on a comparatively large scale, and they thus rendered an immense service to navigation. HARRISON took three years to construct each of his two latest timekeepers; KENDALL took the same time to duplicate

(1) There are no jewels in LE ROY's timekeeper. Though jewelled pivot holes were used in England ever since their invention by FACCIO in 1703, they remained practically unknown on the continent until the very end of the century (see GOULD, p. 98).

number 4; FERDINAND BERTHOUD made more than seventy machines in forty years; but even at the end of his career, he was not able to produce more than two or three per annum. LOUIS BERTHOUD produced 150 chronometers in 27 years. ARNOLD and EARNSHAW produced *thousands* of them. The secret of this considerable increase was simply the division of labour; they themselves did nothing but the final springing and adjusting. The various pieces were made by separate workmen. Of course, this implied a simpler design. ARNOLD was himself a mechanic of uncanny ability. His first chronometer dates of 1770.

7. THOMAS EARNSHAW (b. Ashton-under-Lyne, 1749; d. London, 1829) devised the chronometer escapement and compensation balance precisely as they are now used (1783). He wrote a sort of autobiography called *Longitude, an Appeal to the Public*, 1806.

8. Of ABRAHAM LOUIS BRÉGUET (Neuchâtel, 1747-Paris, 1823), « the STRADIVARIUS of watchmaking », but little is said, for his more original work was done in another direction..

This series is not, by any means, complete, but sufficiently representative of the heroic age of chronometer making. The first satisfactory marine timekeeper had been constructed in 1759; the first chronometer in 1766, but these were isolated individuals, extremely difficult to duplicate. By 1783 EARNSHAW and ARNOLD w re able to produce reliable instruments in larger quantities. That is, in less than twenty five years (1759-1783) the chronometer had become a commercial reality. However, it took *twice longer* to overcome completely the conservatism of ship owners and the recklesness of sailors, though the introduction of the chronometer decreased considerably the risks of their profession!

Here is a good instance of the incredible callousness which impeded this wonderful discovery : « When informed in 1823 by the captain of one of their ships that he thought it expedient to purchase a chronometer for $250, Messrs. BRYANT and STURGIS of Boston replied that he must foot the bill himself, adding : « Could we have anticipated that our instructions respecting economy would have been so totally disregarded, we would have set fire to the ship, rather than have sent her to sea » (1). It was only in 1825 that chronometers were generally issued to ships of the British navy, and, for many years later this was restricted to one chronometer per ship. Mark the dates, 1823, 1825 !

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(1) Quoted by GOULD, p. 132, from S. E. MORISON. *The maritime history of Massachusetts*. Boston 1921.

The second part of the book (p. 134-272) I will not attempt to summarize. It is devoted to the study of the improvements introduced between 1800 and 1920. These improvements, affecting chiefly the escapement, the balance spring and the balance, are very great, but difficult to analyze. Chronometers of 1800 and 1920 may seem extraordinarily alike to lay observers, yet there is as much real difference between them as between HARRISON's and EARNSHAW's timekeepers. For a technical account of this difference, based on hundreds of details, I can but refer to Comm. GOULD's invaluable book.

The author acknowledges his indebtedness to previous works (1), but his own is nevertheless the first of its kind. It is largely based on a careful study of the timekeepers themselves, and secondarily an many unpublished documents. It is richly illustrated with a number of portraits, photographs of instruments and drawings. A glossary of technical terms will be useful to many readers. Though indifferently written, it is a capital addition to the history of technology.

GEORGE SARTON.

Neuburger, Dr. Albert. — *Die Technik des Altertums*. Zweite verbesserte Auflage, Mit 676 Abbildungen. Leipzig, R. VOIGTLANDERS Verlag, 1921, 8°, XVIII u. 570 S.

Neudeck, G. — *Geschichte der Technik*. Mit 550 Abbildungen. Stuttgart und Heilbronn, Verlag WALTER SEIFERT, 1923, 8°, 3 Bl. u. 490 S.

Diese beiden Werke, die sich durch gute Ausstattung auszeichnen, möchte ich hier kurz anzeigen, nicht um sie zu empfehlen, sondern um davor zu warnen, damit nicht das Ausland sie zum Massstab nehme für deutsche Forscherarbeit. Die zweite « verbesserte » Auflage des NEUBURGERschen Buches, das von F. M. FELDHAUS und H. MÖTEFINDT in den *Geschichtsblättern für Technik und Industrie*, VI, 1919, S. 120-137, einer sehr eingehenden kritischen Analyse unterzogen worden ist, weist in der Neuauflage genau eine Seite Text mehr auf. Worin die Verbesserungen bestehen sollen, ist nicht festzustellen, denn all die zahllosen Fehler sind unverändert stehen geblieben, keine der beanstandeten Lücken ist ausgefüllt worden. Das Fazit der FELDHAUS'schen Kritik besteht nach

(1) Such as : BERTHOUD's works mentioned above ; FREDERICK JAMES BRITTEN, *Old clocks and watches*. London 1899, 3e ed., 1911 ; CHARLES GROS, *Échappements d'horloges et de montres*, 2e éd., Paris 1913 (*Isis*, II, 291).

wie vor zu Recht : « Für wissenschaftliche Zwecke ist das Buch von NEUBURGER völlig wertlos. Laien werden von der Antike ein schiefes Bild bekommen, wenn sie dies Machwerk benutzen und... NEUBURGERS antike Technik für ernst nehmen ». Immerhin hat NEUBURGER sich wenigstens bemüht, eine Menge Material zu sammeln, und die Abschnitte, die ihm als Chemiker am besten lagen, sind lesenswert. Auch zeichnet sich das Buch durch eine übersichtliche Gliederung des Stoffes aus.

Von dem zweiten hier angezeigten Werk, dem von NEUDECK, kann man nicht einmal dieses sagen. In skizzenhaften, ohne inneren Zusammenhang willkürlich aneinandergereihten Abschnitten stellt dieses Buch mit dem anspruchsvollen Titel einen Wissensstand dar, der etwa dem von 1880 entspricht. Die neuere technohistorische Forschung, die in den letzten 20 Jahren durch TH. BECK, H. DIELS, F. M. FELDHAUS, H. TH. HORWITZ und andere in Deutschland einen ausserordentlichen Aufschwung genommen hat, ist dem Verfasser unbekannt geblieben. Nicht nur, dass infolgedessen wichtige Forschungsergebnisse, an denen eine « Geschichte der Technik » unmöglich stillschweigend vorübergehen darf, vollkommen fehlen, der Verfasser hat noch dazu kostbaren Raum seines Buches mit Dingen gefüllt, die in einer Geschichte der Technik gar nichts zu suchen haben, wie ein Kapitel über LUTHER, COLOMBUS, USW., oder über die durchaus problematische Glazialkosmogonie von HÖRBIGER. Längst von der Forschung aufgeklärte und richtig gestellte Legenden wie die angebliche Erfindung des Kompasses durch FLAVIO GIOJA oder die Dampferfahrt PAPIN's, tauchen wieder auf, wie überhaupt das ganze laienhafte Buch von Fehlern und schiefen Darstellungen wimmelt. Ganz naiv sagt z. B. NEUDECK (S. 156), über die Dampfmaschine PAPINS, « ob die Maschine, die um 1690 bekannt gegeben ist, wirklich gearbeit hat, ist mit Sicherheit nicht ermittelt ». Hätte der Herr Verfasser sich bei MATSCHOSS oder FELDHAUS über die Geschichte der Dampfmaschine unterrichtet, dan hätte er das wohl nicht schreiben können. Dass NEWCOMEN und JOHN CALLEY, der übrigens nicht Glaser (glazier) sondern Viehzüchter (grazier) war und nicht Cawley sondern Calley hiess, nach den Vorschlägen von PAPIN arbeiteten, ist Phantasie des Verfassers. Sie wurden vielmehr durch SAVERYS Dampfmaschine zu ihren Verbesserungen angeregt. Das äusserst dürftige Kapitel über Luftschiffahrt ist ganze 2 Seiten stark! Papyrus wurde nach der Ansicht NEUDECKS aus den Blättern (!) der Papyrusstaude hergestellt! Dass HUYGENS falsch geschrieben erscheint, wird nicht wundernehmen; aber die « Aeolopile » des alten HERON (von PHILON scheint NEUDECK noch nichts gehört zu haben) hätte er doch

auch aus älteren populären Schriften in der richtigen Schreibweise abschreiben können. Es hiesse ein ganzes Buch schreiben, wollte man auf alle Fehler und Mängel dieses Werkes eingehen. Das Bildermaterial entstammt zum grossen Teil Industrieprospekten und hat oft gar keinen Zusammenhang mit dem Text. Das beste am dem Buch ist die Ausstattung. Schade um die Mühe, die der Verleger hier auf ein untaugliches Objekt verwandt hat.

München. Graf CARL v. KLINCKOWSTROEM.

Katalog der Bibliothek des Reichspatentamts. Stand vom 1 Oktober 1923. 3 Bände in gr.-8°. Berlin 1923.

Band 1 dieses umfangreichen, seit 1913 (1) zum ersten Mal wieder in Neubearbeitung erscheinenden Verzeichnissen enthält das Standortsverzeichnis in systematischer Anordnung. Band 2 und 3 das Autoren- und Schlagwortregister. Die Bibliothek des deutschen Reichspatentamts in Berlin ist die umfassendste technische Bücherei Deutschlands. Sie enthält z. B. eine lückkenlose Sammlung aller gedruckten Patentschriften der Welt. Der Oberbibliothekar des Reichspatentamts, Dr. PAUL OTTO, hat nach dem Vorbild amerikanischer Bibliotheken bei der Ausarbeitung des Kataloges besonderen Wert darauf gelegt, dass sich jedermann darin mit Leichtigkeit zurechtfinden kann. So ist est z. B. durch die möglichst weitgehende Auflösung aller Bücher- und Zeitschriftentitel in Schlagworte vermittelst des Schlagwortsverzeichnissses ermöglicht, ohne Schwierigkeit die gesamte vorhandene Literatur über ein spezielles Thema zu erreichen, auch wenn man vorher weder Autoren noch Titel kannte, während es bei den Katalogisierungssystemen der grossen Bibliotheken sonst kaum möglich ist, etwa die periodischen Veröffentlichungen einer gelehrten Gesellschaft zu finden, wenn man deren genauen Titel nicht kennt. So findet der Benutzer das « Journal of the Franklin Institute » unter dem Schlagwort « Franklin Institute », auch wenn er nicht weiss, ob die Zeitschrift « Journal » oder « Proceedings » oder « Transactions » heisst. Weiterhin ist der Gebrauch des Kataloges auf Grund langjähriger praktischer Erfahrung wesentlich erleichtert durch zahllose Verweise auf synonyme oder sachverwandte Objekte. So finden sich z. B. ausser den 88 Schlagworten unter « Pumpe » (und damit zusammengesetzten Worten) noch 34 Verweise auf spezielle Pumpenarten. So ist der Patentamtskatalog zugleich für jeden, der auf technischen Gebieten literarisch arbeitet, ein wichtiges Nachschlagewerk und Hilfsmittel.

München. Graf CARL v. KLINCKOWSTROEM.

(1) For the 1913 edition, see *Isis* II, 290 (Ed.)

ERRATA

(*For previous errata, see Vol. V, 569.*)

Vol. V, p. 14, l. 10 from bottom read « thought ».
p. 14, l. 2 from bottom read « senses ».
p. 136, l. 6 from bottom read « Containing a bibliography in the order of publication, fairly complete but, in my opinion, confusing ».
p. 213, second note read *Romanic Review*.
p. 210 l. 7 of note on HOLMYARD, read « matter. He ».

A few other misprints are not mentioned in these errata, because they are too obvious to cause any error or confusion. I wish to express my thankfulness to the readers who take the trouble to make the above-mentioned corrections in their set of *Isis*. I would advise them, after having accomplished that little task, to write their initials near mine at the bottom of this note, thus to indicate that these and the previous errata have been taken into account.

G. S.